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ORIGINAL COMMUNICATIONS.

"Qui Docet Discit."

ART. I.—NOTES ON THE RESPIRATORY CHANGES. By RUFUS KING BROWNE, M. D., of New York.

It is not my essential purpose to enumerate errors in physiology, but to point out facts which have hitherto been unapprehended. I shall not specify *exceptional* phenomena, but *all* of those involved in a certain division of the respiratory process. For several years I have not had a moment's doubt that in no text is there a representation of the true character of the facts, or of the reality of the respiratory changes. And this is sufficiently plain, if we, even for the time being, forego the silly habit of iteration of phrases which signify no corresponding reality in *rerum natura*, and take an intellectual look at the facts themselves. The respiratory changes are always said to be phenomena of the "diffusion of gases," but they are *also* called phenomena of osmosis, and that, with both together —though one set of phenomena are different from the other, —they are said to explain the ingress and egress of gases to and from the blood.

What is the capital and *inclusive* fact in the first named phenomena? Simply and notably this, namely: that gases occupying a confined space on either side of a partition of membrane, or certain other substances, will *not continue apart.* They penetrate each from its own side to the other side of the partition and mingle *together.* They cease each to occupy merely the space on one side of the partition, and occupy the whole partitioned space. This is a simple act of *diffusion.* It involves no identity of either antecedent or consequent, and no continuity of process of which it is a variation. And it is a phenomenon which *never* arises in the nature of organic processes. This phenomenon involves, as an integrant of its character, *no* regulative action. It involves no *separation* of gases, but, on the exact *contrary*, its very identity—which is its total character, or all there is of it—consists in its overcoming the preärranged means of separation. In short, the act is simply a doing of the exact opposite of *separation.*

But, on the other hand, this very separation of gases, which in diffusion is completely overcome, or entirely *undone*, is precisely the characteristic phenomenon of the respiratory process. So that precisely what you did to begin with in the former case—namely, to *separate* the gases—is immediately undone, thus enacting diffusion, while the exact contrary of diffusion—namely, *separation*—is what is continually or incessantly being done in the respiratory process.

These two, then, are acts of a different character, even if we regard them as both alike *physical* transactions. In the one case a *diffusion* is not prevented by the space and gas-dividing membrane; but, in the other, a complete *separation* is not prevented. But in the respiratory transaction there is no commingling of gases, *and all there is, common to the two, is a membrane*, which, in both cases alike, is passed or gone through by gases, and in neither case has power to prevent this or to confine the gas.

Now, it is just in this particular, which exists alike in either case—namely, that there is a *membrane* traversed by gases—that the entire mistaken supposition of the likeness or identity of the respiratory changes and the phenomenon of "diffusion" arises. But let us mark how egregious a blunder we have held in the supposition which assumes, (without any further consideration of the respiratory processes in themselves—upon a membrane and gases moving through it, both in this process and the act of diffusion of gases), that the two are similar, or that the one explains the other.

In fact, on a moment's reflection, it is seen that the membrane supposed in the one case to establish the similarity of character with the other, or to make it the same kind of a transaction, so explaining it, does not of itself constitute either the *act* or a *condition* of the act in that case, for it is plain that the membrane does not effect or enact the *diffusion*. That act is not the doing of it. Neither is it a condition of diffusion, for its status is the *contrary*—is, in short, a part to keep separate the gases. This is the sole character of its participancy in the experiment. It is the physical character of the gases which does the transaction, and the act is theirs. The total significance of its interposition being *not* to contribute any character to that act; not to do anything but to artificially demonstate that it *does not* intercept the diffusion of the gases. In fact, it has no power to separate them in diffusion, and yet this very separation, or maintaining of them distinct, is precisely what is done in the respiratory act.

It cannot, therefore, but be a monstrously egregious blunder that, because in the two cases there is a membrane, we should identify the transaction or act in the two cases, seeing that the membrane is not what does the acts, nor indeed is a condition of these acts positively considered, but only shows experimentally that it fails to, or does not, intercept the passage of the gases.

On this blunder, therefore, of indentity of the diffusion of gases and the respiratory changes of oxygen and carbonic acid, on account of the existence of a membrane alike in either, we have entirely missed a true perception of the *acts* in the latter case, or, in other words, the facts in the latter have continued unapprehended.

Let us, therefore, take a closer look at the various acts of the transaction in the respiratory process. As we shall strictly confine ourselves to showing, briefly, the striking unlikeness, and, in part, oppositeness of the *acts* in the two cases which we have in physiology mistakenly identified, we shall not particularize the great differences between the character of the tissue in each and the substances beside tissue in each, because these in both cases are only incidental to, and not constituents of, the acts in question; for it is by examination of these *acts* alone that the truth may be discerned. Brevity will constrain us to enumerate only commanding features.

First—The respiratory process involves a mesh of minute tubes filled with moving blood-globules and blood-liquor. Diffused in the latter is a certain amount of carbonic acid gas. The wall of the blood-tube is one thickness of tissue; upon it another thickness, consisting of a layer of epithelial cells. In contact with these is the heated air. These two thicknesses supposably correspond to, but really are extremely unlike the partition membrane in the case of diffusion of gases. A certain *per centage* of a component gas of the air is withdrawn from the latter, being on the airside of the partition, by, not the blood-fluid, but the blood-globules on the other or inner side.

This is a distinct act of *separation* of a definite quantity of gas; and not only that, but a separation of it from a very large proportion of the same gas left behind, and an appropriation of it by certain anatomical elements. This gas does not diffuse, but is carried away by these elements.

On the other hand, a certain per centage of a gas, suspended in the blood-liquor, *separates* from its menstruum and passes from the inner to the outer side of the partition. This, also, is an act ot separation, which precludes there being either mingling or diffusion of these gases. In the one case, the gas which has been taken from the outer side to the inner side—*i. e.*, into the blood—has been exclusively "separated." And in this character lies the very identity of the phenomenon—that which individualizes it from the phenomenon of diffusion.

Why, then, do we identify the two different phenomena? The answer to this question is not difficult. But it involves a statement of the already exposed blunder. We identify the two *because*, in both cases, there is a membrane and a gas moving through it. But in reality these factors in the case do *not* constitute the *act*, but only the similar substances. One of them, we have seen, takes no part in the act in either case, namely, either in diffusion or respiration; and the other shows the physical *characters* of substance concerned alike in both, but not, in this case, the character of the *act or transaction.*

But, further than this, substances of physical characters, which impel to diffusion, *will* not pass through a membrane in the respiratory process, showing that the act is not to be identified with the physical characters of *diffusion.* In this instance there is an exclusion of the phenomenon of diffusion, for the act of respiration—the ingress of oxygen—will go steadily on, while not an atom of the highly diffusible substance, on one side of air in contact with the membrane, will pass through it. This is the case notably with hydrogen. It is excluded from taking part in the respiratory acts, though the most diffusible of all gaseous substances, and though through the membrane other gases less so are passing out and in, while it occupies a place against the one side of the membrane, it will not diffuse; will not pass to the other or inner side. Yet, this is not an *exceptional* case, for if artificially introduced and diffused through the blood-liquor it will pass *out* through the membrane rapidly.

The respiratory acts, therefore, cannot be identified, without obliteration of the whole characteristic phenomenon, with acts of "diffusion." For not only do the former differ from the latter in the particulars we have enumerated, but they will proceed independently of the latter. Acts of diffusion cannot be respiratory acts if the most diffusible of all gases does not pass through the respiratory partition.

But enough has been said to make it plain that in identifying the respiratory acts with the diffusion of gases' which is the very same thing, to the extent that we have called the former acts phenomenon of diffusion, and so represented and taught the facts which *are totally otherwise*, we have utterly failed to perceive the true character of acts of respiration.

But, after all, nothing can present more ample testimony to the manifold unlikeness of "diffusion" and the infusion of gases in respiration than the former itself. We continue to call these the same when a gas, which diffuses above all others, is not able to pass over an *atom* of itself, in a respiratory act, through the membrane likened to the septum in diffusion, thus showing that the transit of a respiratory act is *not constituted* by an act of diffusion. Can any phenomenon exceed this in the force belonging to the character of truth, (as a *demonstration* presented by natural phenomenon,) that diffusion and the act of respiration are as distinct and *as widely variant* as can be? This is not due to the membrane—not operated by any *stopping* power of it, for the gas will rapidly pass through the same membrane from its other side outward; and the same gas, if placed on the pleural side *of the lung*, will pass through in the other or pulmonary cavity side of it. What does this show? It shows that an act of diffusion neither will nor does take place in a respiratory act, and that something distinct from it, namely, a respiratory act, will regularly and amply take place where an act of diffusion does not occur; and this diffusion (when if the two were the same) could not be and occur, while in that occurrence a respiratory act would be effected; would, in short, be identified with that act, and *in* it and the respiratory act *be* one of diffusion. What *constitutes* the respiratory act would, then, be an act of diffusion. But no such thing takes place. But, on the exact contrary, in the taking place of the respiratory act, and the failure of diffusion, under the circumstances it *exhibits its characters* palpably, even to the observing senses, establishing the difference between it and mere diffusion by all the breadth to which *difference* can extend. We have thus nature's own exhibit that the first is another kind of act from the second—an exhibit which shows their diversity, so showing our false supposition of sameness to be utterly imaginary.

Unlike as these phenomena are, and we have not pictured this unlikess to near its fullest extent, nobody will pretend that the respiratory acts take place without a gas, or without a membrane traversed. In respiration, however, the membrane does not exist for any purpose having the slightest relation to diffusion.* It exists solely as an anatomical necessity for confining the blood her-

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*It is an overwhelming illustration of the invariable puerility, not to say silliness of our notions, *in the absence of knowledge*, that no one has intellectually discerned and formulated the true import of the septum used in experiments of diffusion. Nobody has had the faintest breath of suspicion of the truth that *we* fix this membrane amid the diffusive gases in order to show that, despite it, the gases will unite, and it shows what the membrane *will* metically in the body. A gas which enters the blood must, therefore, in obedience to this necessity of structure, enter it through the walls or membrane of its channels. But nothing, in the least characteristic of the respiratory act, takes place therein by *means* of this membrane. It has no *power* to accomplish the movement of the gas through it. It is only what is within and distinctly different from the membrane, namely, the red-globules, which appropriates the gas which enters.

If the facts have been clearly presented, we have made evident:

First—That the phenomena enacted in that division of the respiratory acts which involve oxygen and carbonic acid are in no respects acts of "diffusion" of gases. For they consist (1) of the separation of gas from a fluid which holds it; (2) of the separation of a definite proportion of another gas, oxygen from the air and its introduction into the blood.

Second—On neither side do these two gases, each traversing the membrane, in pursuance of *diverse* functions of the economy, mingle as in cases of diffusion, for (*a*) one of them is appropriated by one part of the blood solely, namely its globules, and (*b*) the other is yielded by another part of the blood, namely the blood *liquor*.

ART. II.—CONCUSSION OF SPINE, FOLLOWED BY PARAPLEGIA —EFFECTS OF TREATMENT BY BROMIDE OF POTASSIUM.

By Henry F. Andrews, Washington, Ga.

Mr. S——, a very robust, healthy man, about sixty-five

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not do. Now, whatever the similarity we imagine between this septum and membrane and in organic forms, anatomically arranged, nobody, once apprehending the case, would ever suppose, in the latter case, a membrane is ever arranged in order anatomically to do the *same* thing, namely, to show what it will *not* do. This is an example of the innumerable fallacies we commit, and c all knowledge by *identifying* in mind experiments with natural phenomena to the extent of making the former actually one in character with the latter.

years of age, was thrown from his buggy in September, 1867. He was taken up in an insensible condition, and remained so for several hours. He gradually recovered consciousness, and, in a few days, was up and about, though from the time he recovered consciousness he complained of pain in the left foot. This pain gradually becoming worse, and swelling, with considerable loss of power, in the left lower extremity coming on, he was compelled to return to bed. The limb continued to swell and the pain increased till it became very severe.

The limb was bandaged with a flannel roller, which reduced the swelling considerably; but the right foot then became painful and the right lower extremity also swelled and became powerless. I saw the patient for the first time on December 29th, in consultation with his attending physician. At that time he was in a most pitiable condition. He was lying upon his back, unable to move; both lower limbs paralyzed, swollen and excessively painful, the pain aggravated greatly by the touch. The right, at this time, was more swollen and painful than the left. Both limbs were bandanged with flannel rollers, which seemed to give comfort and reduce the œdema.

The spine was excessively tender in the lumbar region, though I was unable to examine it personally, on account of the impossibility of turning him in bed, in the least, without giving him such excruciating pain, that I was unwilling to subject him to it.

He had been treated by mercurials, and was slightly ptyalized when I saw him. He was kept constantly under the influence of opiates to relieve pain and procure sleep. His pulse was full and frequent, tongue dry and brown, and skin yellow, almost jaundiced.

My diagnosis was concussion of the spine, followed by inflammatory action and paraplegia. Prognosis extremely unfavorable.

I advised the opiates to be left off entirely and at once, and that the bromide of potassium be given in fifteen-grain doses three times a day, with one or more extra doses at night to procure sleep, if necessary. I was in hopes that it would produce sleep and quiet and relieve the pain, thereby doing away with the use of opium and its disagreeable and, in my opinion, injurious effects. I had also some slight hopes that it might have a beneficial effect upon the disease.

My directions were carried out faithfully, the patient leaving off all other internal remedies. The good effects of this treatment surpassed my expectations. It produced sound, quiet and refreshing sleep. The opium was left off without difficulty. There was soon, and has continued since, a marked improvement in all his symptoms.

The pain and tenderness in the spine have greatly diminished; the swelling in the extremities has subsided, and a very considerable amount of power has returned to them. His appetite, digestion and all the secretions are now in a good condition. He has been able, since about the first of March, 1868, to leave his bed and sit most of his time in a chair, and is steadily improving.

I can scarcely, even now, hope for complete recovery in this case. I think, however, had the bromide been given earlier, before any serious change had taken place in the spinal cord or membranes, there would have been a fair prospect of complete and speedy recovery. I can but attribute his great improvement to the bromide, for up to the time of its administration he had been getting steadily worse, until he had reached a point where death seemed not far in the future.

Art. III.—Translations from Foreign Journals. By A. M. Fauntleroy, M. D., Staunton, Va.

A Curious Case of Fecundity.

The following case, reported at length by M. Galopin,

D. M. P., (Revue Therapeutique, Medice-Chirurgicale, 1867, Paris,) is interesting in an obstetrical point of view from its extreme rarity, but especially is it worthy of notice from physiological considerations, demonstrating the wonderful "nisus formativus," notwithstanding the multiplicity of the fœtuses.

The subject was forty years of age; this completed her seventh pregnancy.

At five months and a half she was observed to be as large as those at nine months ordinarily attain. At this period she was accouchée and gave birth *to five male children*, all of whom were as well formed as is ordinarily the case with an infant conceived alone, and with five and a half months of intra-uterine existence.

The accouchement was accomplished with scarcely any difficulty. The children lived from four to seven minutes, and were successively baptised whilst living.

There were five umbilical cords inserted into two placentas, which adhered slightly at one part of their circumferences, three cords being attached to one and two cords to the other.

A MEMOIR UPON THE VALUE OF ARSENIC IN THE TREATMENT OF PULMONARY PHTHISIS,

Read before the Academy of Medicine, Paris, by M. le Docteur Moutard Martin, Medicin de l'Hospital, Beaujon. The following are the conclusions:

1. Arsenical medication has an effect very marked upon phthisis pulmonalis.
2. Its efficacy is more manifest where the progress of the disease is slow and sluggish than in phthisis accompanied by fever.
3. Phthisis, rapid in its march, or acute in manifestation, is not atall modified by arsenic.
4. In a large number of cases, even advanced into the

state of hectic fever, the general condition of the subjects may be favorably modified for quite a lengthy period.

5. The modifications of local lesions are only produced very slowly.

6. A number of recoveries should be attributed to arsnical medication, which would have accomplished more if those patients, who fancying themselves well, had not desisted in the medication.

7. In order to be efficacious, the treatment should be continued for a long time.

8. The arsenic should be administered in fractionally small doses.

9. The daily doses should not exceed two centigrammes, (equivalent to two-tenths gr. apothecarie's.) [M. Moutard Martin employs arsenious acid in granules of one millegramme each. Commencing with one or two millegrammes he gradually increases the dose by granule, according to the tolerance of the individual.]

10. Arsenic is better borne by those in whom the disease has made but little progress than by those far advanced in consumption.

11. When the doses can be augmented from 15 millegrammes to 2 centigrammes, the tolerance may be, so to speak, unlimited.

12. The arsenical medication is primarily restorative in its action and secondarily modifies the pulmonary lesion. However, certain facts show that arsenic exercises a direct power over the respiratory function, and acts, doubtlessly, upon the pulmonary tissue itself and upon the tubercule. —[*Le Mouvement Medicale*, 1868.

DE L'ADMINISTRATION DES MEDICAMENTS PAR LA MUQUEUSE NASALE.

By Doctor Raimbert, Revue de Therapeutique, Médico-Chirurgicale No. 4, 1868, Paris.

The pituitary membrance, as a medium of absorption or introduction of medicines into the system, is at the present day so much neglected, that this contribution is made with a view to enforce the propriety of the procedure. The design appears to have been, in dealing with this membrane, to stimulate and excite, so as to provoke the reflex action of sneezing; whence the name *sternutatoria* given to medicines, either simple or compound, which have been introduced into the nasal fossæ.

The salivation which we have observed frequently after several days employment of a powder, composed of calomel, red precipitate and sacchari albi, against ozæna, [Trousseau's formula], demonstrated the facility with which the pituitary membrane absorbed; and suggested the idea of resorting to this way of absorption in order to counteract painful affections of the head, and those of the eye.

Our experience has not been large; the recorded observations are neither numerous nor varied, and we should have refrained from their publication, until we could have gathered facts for a more enlarged report, if we had not observed in a number of the Gazette des Hópitaux, of April 18th, 1867, an article by M. le Docteur Noel Gueneau, of Mussy, which pertained to the subject, which we have had under research for more than six months. But incomplete as the report may be, we have thought the facts already attained of such utility as to warrant their publication.

Here is the resume of our observations:

Obs. I. M. H——, aged twenty years, suffering for several days with influenza, with coryza. He experienced, the 16th January, an extremely intense pain in the left supraorbital region. The pain continued for 24 hours, and increased at times, depriving him of sleep. The breathing through the nasal fossæ being unobstructed, we prescribed, powder of mallow, 15 grs.; muriate of morphia, gr. 1-10; directed a pinch of this powder every two or three hours.

In the evening the pain had abated; passed a comfortable night, and in the morning, upon awakening, all pain had gone.

Obs. II. The twenty-fifth of January, I——, aged sixty-five years, was admitted to the hospital, with a grave bronchitis, which necessitated the application of a large blister between the shoulders. In the first days of February, when convalescent, this female was seized with an acute cephalalgia. The pain of the head having resisted the mustard pediluvia, we ordered the following powder: Sacchari albi, 30 grs.; Morphiæ muriatis, gr. ½; pinches to be taken, as snuff, every two or three hours.

In the morning the pain had diminished; upon the third day had entirely disappeared.

Some days thereafter the cephalalgia returned, and the same prescription caused its definite disappearance.

Obs. III. A girl, sixteen years old, entered the hospital with chlorosis, complaining of acute pains in the head, which evinced a tendency to become intensified in the afternoon.

In connection with iron, we prescribed, sacch. albi, 30 grs.; morphiæ muriatis, gr. ½. Directed pinches, by nasal inspiration, to be taken every three hours during the morning and every two hours of the afternoon.

In the morning slight abatement of the pains; the pinches of the powder, to be used as before, every hour; the amelioration became more pronounced, but it was only about the sixth day that entire relief from pain was secured, and the cure was completed by the iron administered.

Obs. IV. The 10th of February, was called to the bedside of M'lle Est——, aged seventy-six years; she was affected with the influenza, with coryza so slight as not to obstruct the nasal fossæ. The patient was rheumatic, and complained of shooting, sharp pains over the left side of the head, and of a buzzing noise in the ear. These pains existed in the right side but were less marked.

Sacch. albi, 30 grs.; morphiæ muriatis, gr. j; this powder was exhibited, by nasal inspiration, every two or three hours.

In the morning the pains had vanished, the noise or humming in the ear alone remaining.

Obs. V. S——, aged sixty-two years; had suffered for two months or more with neuralgia of the inferior maxillary bone, upon the right side, which still continued. The pain embraced the gums and the right lateral half of the tongue; had already had several teeth extracted, but the pain continued.

The most painful points of the gums and buccal mucous membrane and the side of tongue was lightly touched with nitras argenti; subsidence of the pains only transient; a blister behind the right ear failed to remove the pain.

We prescribed the same powder, as in case 4, and at like intervals. A remission and diminution of the pains, very decided, was attained, and her condition was comparatively comfortable.

Obs. VI. B——, neuralgia in the teeth of the side of the inferior maxillary bone. We prescribed: Sacch. albi, 30 grs.; morphiæ muriatis, gr. ½; to be taken by nasal inspiration, in pinches, frequently during the day. Little, if any, relief was afforded. We changed the proportions of the powder, ordering: Sacch. albi, 30 grs.; morphiæ muriatis, gr. j; to be used six times during the day. The pain subsided and did not again return.

We should be able to add greatly to the number of our reported cases, if the patients, living in the country, to whom we have given like powders could have given us the result of the medication, but, as they did not return, it is fair to presume that they were relieved. The first trials of the powder were not at first complete in the relief afforded, but doubtless this was due to the fact that the powder administered contained the morphia in too small a proportion. Little by little we have diminished the quantity of sugar, until a happy combination was attained: Sacch. albi, 30 grs.; morphiæ muriatis, gr. j.

Before administering the powder it is necessary that any mucous or scurf, which may be present in the nasal fossæ, should be removed.

The circle in which we have employed our means might doubtlessly be very much extended. The pains of irido-choroiditis, photophobia, are, we believe, controllable by narcotic preparations introduced in the form of powders, by inspiration, to the pituitary membrane.

Other medicines should also be employed through this channel, as the powder of nux vomicæ, or its alkaliod, strychnia, etc.

We have attested the presence of the iodidum potassii in the urine, in about two hours after five grains of this salt had been nasally inspired in connection with sugar, and this mode is far superior to the frictions with the ointment of the iodide of potassum over the temples or sapra-orbital region.

PHYSIOLOGICAL AND THERAPEUTICAL ACTION OF VERATRUM VIRIDE. Revue de Therapeutique Médico-Chirurgicale, 1868.

M. le Docteur Oulmont read before the Academy of Medicine, Paris, a lecture upon the "Physiological and Therapeutical Action of Veratrum Viride." He instituted a series of experiments upon rabbits, dogs and frogs to test the degree of its physiological activity, and the result of this experience is made the ground-work of his lecture. After a careful study of the article, both botanically and pharmacologically, M. Oulmont relates his experience, of which the following facts are in résumé:

Veratrum viride, administered in non-toxical doses, exercised a multiple action particularly localized upon the *intestinal tube, the respiratory and circulatory functions*, and upon the general forces.

Nausea is rapidly induced, violent vomiting, which continued sometimes from fifteen to twenty hours; diarrhœa was marked. When the animal died no trace could be found of inflammation along the gastro-intestinal tract. The action upon the digestive organs was manifest even when the medicine was used hypodermically.

The respiration is rapidly and profoundly modified, becoming irregular, sometimes very rapid, and again so slow at times that there were only one or two respiratory movements during a minute; occasionally, among the frogs, respiration was completely suspended.

The circulation was very rapidly controlled. The pulse fell, in the course of fifteen to twenty minutes, 20, 40 to 60 pulsations. *Upon man*, in the febrile condition, the veratrum viride, in doses from 1 to 4 centigrammes*, made the pulse fall 30 to 40 pulsations.

The temperature manifested a retrogression scarcely less marked. In about an hour and a half to two hours, the temperature descended two, three and even five degrees, and continued so without occasioning the animal's death.

The hypostemic action of the veratrum viride manifested itself from the beginning almost; feebleness and prostration gradually increased, and when at their maximum point the animals died.

The tincture of veratrum viride became toxical to frogs in 20 drop doses; to rabbits in 60 to 80 drops, and to dogs in doses of 120 to 130 drops.

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*Centigramme equivalent to 1-10 gr. apothecarie's.

M. Oulmont made a comparative trial of the virtues of the veratrum album and the veratrum viride. After having recalled the fact that these two varieties differed only in a few botanical and pharmacological characteristics, he proved that the action of the veratrum album resembled altogether that of the veratrum viride. The only difference, if such it can be called, was the greater violence of the action of the veratrum album upon the digestive tube, which, under the knife, exhibited always a lively inflammation. Its action was dreadfully quick, the animals died in a much shorter space of time from half the quantity which was required of the veratrum viride to produce death.

In fine, M. Oulmont made a comparative study of the physiological action of the veratrum viride and its alkaloid veratria, and he arrived at the conclusion that this alkaloid, which forms a considerable portion of the veratrum viride, was not the active principle; and this conclusion he confirmed by experiments made upon rabbits with the veratrum viride deprived of the veratria. The experiments elicited precisely the same phenomena as when the veratrum viride was employed pure.*

M. Oulmont concluded from all these facts that the veratrum viride was poisonous to the heart, sedatively analogous to digitalis, though more rapid in its action.

In a future lecture the author will make known the results of his clinical researches.

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*This conclusion is at variance with the views generally entertained.—Translator.

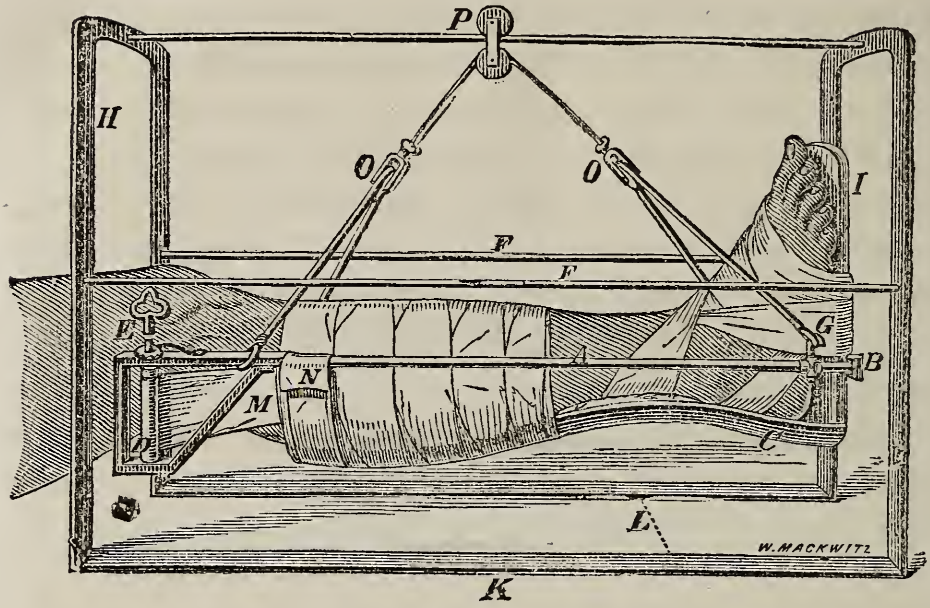
ECLECTIC DEPARTMENT.

"Floriferis ut Apes in Saltibus Omnia Limant."—Lucret.

Art. I.—Clark's Suspension Railway Splint. A Suspension Splint for treating Simple and Compound Fractures of the Leg. By E. A. Clark, M. D., Resident Physician, St. Louis City Hospital.

The great necessity for a well adapted apparatus in treating fractures of the leg, suggested the utility of the instrument I have designed in the following woodcut, which not only answers every practical purpose in treating this class of fractures, but also contributes very much to the comfort of the patient, who, while he is enabled to execute every movement of which the sound limb is capable, yet, cannot displace the fracture or modify the force of extension. In presenting this apparatus, I claim an advantage over those invented by Hutchinson, John Neill, Crandall and Salter, not only for the means of extension and counter-extension, but also its adaptation to the treatment of compound fractures of the leg, as represented in figure No. 2. And, considering the simplicity of this instrument, with its cheapness and application to every variety of fractures of the leg, will certainly give it the precedence with those who may venture to use it in a single case. The apparatus is such as may be made by any blacksmith, or indeed by any ingenious surgeon in a case of necessity, when a wooden frame and two hoops, with a common iron pully, will answer quite as well as the instrument which I have made of iron on the following plan:

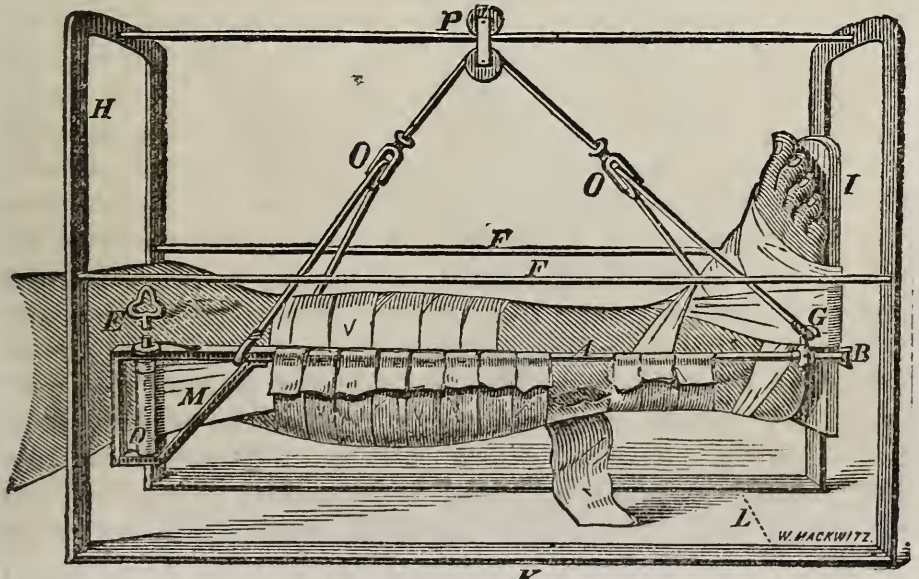
No. 1.



The two arches represented by the letter (H), at one end, are made of iron bars one-eighth of an inch in thickness and three-fourths of inch wide. These arches are continuous with the bottom pieces (K), which support them upon the bed and measure twenty-two inches in length, making the distance between the two arches, which are also supported on the sides by the two slender bars (F F). While the bar extending across the top, upon which the pully (P) glides, should be made flat, with the long diameter perpendicular so as to prevent it bending with the weight of the leg. The width of the arch under which the leg is suspended, as indicated by the letter (L), should be 15 inches and the arch 18 inches from the surface of the bed.

This description will be sufficient to indicate the proportions of the exterior apparatus. The bars represented by the letter (A), in which the leg is suspended, should be about two feet in length—unless when the fracture is too close to the knee, and it may be necessary to attach the adhesive straps (M) above the knee, then the bars may extend to near the perineum if necessary. The cross-bar passing beneath the bracket at (B), and upon which the foot rests, should be flattened and five inches in length, so as to allow ample space for the limb to rest between the bars. The space between these bars at the upper end should ordinarily be about six inches. The splint (C), upon which the leg rests in figure No. 1, should be fluted upon its upper surface so as to conform to the shape of the leg, while it is also made oval upon its under surface, so that both the leg and the splint may be included in the bandage shown in figure No. 1, by which means any displacement may be corrected in the fracture and the bones kept in perfect apposition. The foot-piece (I) should be attached to the posterior splint at an obtuse angle, so as to correspond with the natural position of the foot. The foot is bound to this piece by means of adhesive straps, which may embrace the whole of the foot, and extend partially over the ankle, but not so as to arrest the circulation as by the figure of eight bandage formerly used around the ankle for making extension. The leg then, as seen in figure No. 1, is supported upon the cross-bar passing under the bracket (B) attached to the foot-piece and by resting upon the strap (N), pinned over the bars (A) on either side, while the extension and counter-extension is effected by means of the bar across the foot-piece below and above by means of adhesive straps, three inches in width, as indicated by the letter (M), which are attached to the sides of the leg, beginning just above the point of fracture and passing up to be wound around the cylinder (D), which is three and a half inches in length, and turned by means of an ordinary clock key, represented by the letter (E). This cylinder is held in any position to which it may be turned, by a ratchet and wheel placed upon the upper surface of the bar, as indicated in the diagram—

No. 2.



It will be observed in figure No. 2, that there is no posterior splint as in the other diagram, but the leg is supported entirely by strips of muslin pinned over the bars on either side, which renders this apparatus more appropriate for the treatment of compound fractures in which the wound may be examined and dressed when necessary, by removing one or more of these strips which may be replaced by new ones without disturbing the fracture. The attachment of the foot-piece in this dressing does not in any particular differ from that of figure No. 1. The means of suspension is the same in both these dressings, which, by means of the pully at the letter (P), the patient is enabled to move his limb, or even his body, forward and back to the extent of the length of the bar upon which it glides, and by means of the cord playing over the under wheel in the same pully, the patient is able to flex and extend the knee by depressing or elevating the foot, which movement can be executed by a very slight effort on the part of the patient, while at the same time he can swing the leg from side to side to any extent within the space of the arches; and by means of the cords playing through the pulleys at (O O), the leg can be rotated to any extent, even to allow the patient to lie upon his side if he desires, without disturbing the fracture in the least. It will be observed in the diagrams that at the letter (G) there is a thimble, which can be made to slide upon the bar, by means of which the lower end of the leg, can be elevated or depressed at the will of the patient, by sliding this thimble forward or back, and fixing it at any point by means of the little thumb screw attached to this thimble. In developing the utility of this apparatus for the treatment of fractures of the leg, I have tried various means of attaching the foot at the bottom, such as the muslin and flannel bandages in the form of a figure of eight around the ankle, covering the foot also, as far as the toes; but have always found them objectionable from the great amount of pressure and consequent arrest of the circulation in the foot, though the flannel bandage is much less objectionable than the muslin. But I have been able to obviate this objection by the use of the adhesive plaster attached over the front of the foot and around the foot-piece, as shown in the diagram; this I have ordinarily found quite sufficient, unless in rare cases, when an unusual counter-extending force is required, it may become necessary—as very aptly suggested by Prof. Hammer of this city—to pass a strip of adhesive plaster beneath the heel and around the foot-piece, which adds very much to the strength of the dressing. I have recently treated six cases of fractures of the leg with this apparatus, in which both bones were fractured, and in which there was more or less shortening in each case, with excellent results in all of them, without allowing the least deformity or shortening, while the patients were all grateful for the comforts allowed them by this apparatus during their confinement.

Art. II.—Chloroform in the Treatment of Impacted Gall-Stones. By Roberts Bartholow, M. D., Professor of Materia Medica and Therapeutics, in the Medical College of Ohio, etc.

In the American Journal of the Medical Sciences for July, 1867, may be found a paper by Dr. Thos. A. Buckler, of Baltimore, on the use of chloroform in the treatment of gall-stones. In this article Dr. Buckler claims that "chloroform taken into the stomach for the solution of gall-stones contained in the gall-bladder, and the continued use of succinate of iron to control the fatty or cholesteric diathesis, and thereby prevent the formation of other calculi, have been invariably found by him trustworthy and successful after an experience of twenty years." He states, also, that so long ago as 1848, he ascertained that gall-stones "subjected in several vials, to every agent deemed capable of exerting on them a solvent influence" without effect were finally dissolved in Edinburgh chloroform "in a few minutes, leaving only a friable mass resembling the cinder of burned paper." He remarks further: "It is above all other agents the remedy for this disease, performing as it does, the double work of a solvent and an anæsthetic. * * After what has been demonstrated by H. Bence Jones and others in reference to the permeability of the tissues, there is every reason to believe that a sufficient quantity of chloroform would pass into the gall-bladder through the intervening parts alone to dissolve out gall-stones, provided its uses were continued a sufficient length of time; but when we reflect that chloroform is taken directly from the stomach into the current of the circulation, and that a large portion of it is carried directly to the acini of the liver, where, mingling with the newly formed bile, it passes with it into the gall-bladder, it is easily seen that gall-stones may be dissolved with as much certainty as if they had been placed in a bath of chloroform outside of the living body!"

Without denying the priority in these investigations thus claimed by Dr. Buckler, it is certainly true that he has been anticipated in the publication of them, not only in respect to the clinical facts themselves, but also in the explanation which he proposes of the *modus operandi*. The following extract from Trousseau's Medical Clinic of the Hotel Dieu, (*Tome* 3 *mo. p.* 236), will prove this:

"The famous remedy of Durande, which consists in the administration of a mixture consisting of three parts of ether and two of turpentine, is founded on the principle of the solution of the calculi. More recently physicians have seriously proposed the internal use of chloroform, because M. Gobley has demonstrated that hepatic calculi were more readily soluble in this chemical agent than in any other.

"You know, gentlemen, what I think of chemical theories as applied to the processes which go on in the living body. * * Notwithstanding it is easy to comprehend that in a test tube, by prolonged contact with a menstruum of the maximum solvent power, solution will be accomplished, yet it is absurd to suppose that the same result will be attained when the solvent is so diluted and modified before arriving in the liver to come into contact with the calculi."

To M. Gobley does Trousseau ascribe the discovery of the solvent powder of chloroform, and although the use of this agent appears to be a recognized practice in France, on the theory of its power to dissolve biliary calculi, the distinguished author of the *Clinique Medicale*, believes its virtues to be due to its anæsthetic property. As Dr. Buckler does not appear to have published any previous observations on the subject, it is perfectly certain that our French cotemporaries have not stolen the idea from our American clinician. It is only another instance of those remarkable coincidences in discovery which are found in the annals of science. Nothing is more evident than that the use of ether and turpentine would naturally suggest in obstinate cases, a trial of chloroform. Dr. Buckler and M. Gobley were led by the inductive method to the discovery of the utility of chloroform, and hence it is not necessary for us to assume that one is a discoverer and the other a plagiarist. I trust your readers will believe that that I have looked up the matter solely as an interesting question of medical history, without any design of reflecting upon Dr. Buckler, who is a very distinguished and successful practitioner of Baltimore.

ART. III.—LIME INHALATIONS IN PSEUDO-MEMBRANOUS CROUP. By BEN. B. WILSON, M. D., of Philadelphia.

Since my article on the therapeutic value of lime inhalations in croup, published in a previous volume of the Reporter, was written, I have used the remedy to a very considerable extent in the mixed forms of the disease; where the spasmodic or the catarrhal element seemed to prevail, and always with a result most agreeable to my patients and satisfactory and encouraging to myself. The warm vapor when respired with the atmosphere, seems to have a most soothing effect upon the mucous membrane of the air passages; relaxing spasms, promoting secretion and expectoration, and thus at once relieving the distressing dyspnœa which characterizes the disease. The little sufferer almost invariably drops to sleep while inhaling the vapor; the repose being comparatively calm and undisturbed, and generally accompanied by a profuse perspiration, which evidently contributes to the mitigation of the symptoms in no inconsiderable degree.

No case of genuine and uncomplicated pseudo-membranous croup, had however, occurred to me since writing the communication above referred to, until ten days ago, when, when I was called to a case of several days duration, and in which the symptoms were of the gravest and most promising character; and one eminently fitted for putting the therapeutic virtues of this remedy to the severest test.

I. S., aroused me at 4 o'clock, Monday morning, with a request to visit his son, three-and-a-half years old, sick with croup. The history of the case was that the little boy had sickened with a high fever and hoarseness of voice on Thursday evening, and had once or twice during the day manifested a peculiar loud barking cough, which, though somewhat alarming, had given them no uneasiness, because in other respects the little patient seemed so well, not having for a moment intermitted his accustomed plays. The hoarseness increased, until on Saturday evening the voice was quite whispering, and he spent a restless and most distressing night, having a very high fever and occasional croupy cough. The parents being young and having little knowledge of disease, and having no experience whatever in croup, were not aware of the significance of these symptoms, and as the child seemed better on Sunday morning, no medical aid was called, and some simple expectorant was given.

On Sunday night, however, the severity of the disease became evident even to non-professional observation, and my services were invoked; though in consequence of the patient living a long way from my residence, it was almost morning before I reached the house. I found the little sufferer almost in articulo mortis; speech, except in the faintest whisper, was impossible. The respiration was hurried and gasping, and accompanied by that peculiar shrill whistle on inspiration, which is characteristic of the presence of false membrane. The supra sternal fossa also was very marked and sunken at each recurrent effort at inspiration. The surface was bluish and almost mottled, and the lips and nails quite blue, showing that the æration of the blood was already considerably impeded; and, altogether, the condition of the patient was such that unless relief was obtained death impended, and that at a period not very far distant.

Treatment was at once begun by immersion of the hips and extremities in a hot mustard hip bath, and by active counter irritation, by means of turpentine stupes to the throat and chest, and by the administration every hour of a powder composed of calomel, ipecacuanha and sal ammoniac, the child having been very freely vomited with the compound syrup of squill before my arrival. The administration of the vapor of lime by inhalation was then commenced, and having been perseveringly kept up, notwithstanding the fears and continued opposition of the child, for an hour and a half, we were rewarded by seeing the suffering patient gradually respire with less effort, and fall into a comparatively comfortable sleep. The use of the remedy during sleep became, of course, quite an easy matter, and was continued with scarcely any intermission, a fresh lump of lime being slacked at least every thirty minutes during sleep, and as often during the waking hours as the child could be induced to respire the vapor. During the entire day of Monday little apparent change occurred in the condition of the patient, except that the respiration was markedly less labored than when treatment was commenced. A decided exacerbation occurred during the evening and early part of the night, which was met by the continued and persevering use of the remedies above indicated, with the effect of procuring very decided relief before midnight. The afterpart of the night was spent almost entirely in easy slumber, during which the inhalations were constantly continued. Tuesday morning found the little patient very greatly improved, and on Tuesday evening he was evidently out of danger, though the hoarseness and croupal cough remained for several days, and, in fact, were plainly distinguishable at the end of the week, especially during the spasmodic efforts of laughing or crying.

The other child of Mr. S., a little girl of sixteen months, was attacked with symptoms precisely similar to those of her brother during Sunday night, a few hours before I was sent for. In her case vomiting was freely induced, and a similar treatment substantially proceeded with. She was convalescent on Monday, and rapidly recovered, without catarrhal symptoms.

In my experience the occurrence of two or more cases of *pseudo-membranous* croup in the same family, at the same time, is not a *very* infrequent circumstance. It has happened repeatedly to me. Among patients it gives rise to the surmise that the disease may have a contagious or epidemic origin. Such a suspicion is, however, without foundation, the disease, doubtless, being generally induced by atmospheric changes, and occurs simultaneously in children of the same family, because they are constitutionally and perhaps hereditarily similarly predisposed to be affected by the causes of the disease.

I look upon the case above narrated as contributing materially to establish the therapeutic value of the inhalation of the vapor of lime. The case was one of undoubted pure and uncomplicated pseudo-membranous croup. It had existed for several days and was in a most critical condition when first seen and prescribed for. No physician of experience need be reminded of the grave character of such forms of affection; they are rightly considered almost necessarily fatal, and our weekly mortuary reports sufficiently attest the fact of their frequent occurrence. An efficient remedy for such a desperate phase of disease is a boon to the profession and to humanity, and such the vapor of lime appears to be.

It can be used in addition to and without interfering with any other treatment, and occasions no distress whatever when once the natural fears of the little patient are overcome. On the contrary, the *immediate* effect is always of a most particularly soothing and grateful character. There can, therefore, be no objection to its exhibition in cases in which the prognosis is most unfavorable, and it is in those instances, particularly, that I desire the profession to give it an opportunity to prove its value. Each trial thus becomes an experimentum crucis, which will, I doubt not, soon be followed by the recognition of the remedy as an established therapeutic agent.

As stated in my last article, it can be conveniently administered by slaking in a pitcher a lump of quick lime, as originally suggested by Dr. A. Geiger, of Dayton, Ohio; a vapor bath being extemporised by two or three large quilts or blankets, supported by chairs or props, the head-board of the bed, or arm of the sofa, and including the entire body of the patient within their limits. If hot water is used very active chemical action is at once induced, with the evolution of a good deal of heat, and the throwing of a dense watery vapor strongly impregnated with the hydrate of lime. This vapor surrounds the body, and, mixed with atmospheric air, is respired. It produces, besides its effect upon the air passages, a most profuse diaphoresis, which alone must exercise a curative effect upon the disease. In children of more mature years, or in adults, where a diaphoretic effect is not desirable, an inhaler or generator, with flexible mouth-piece attached, would doubtless prove useful, as applying the vapor less diluted, and more nearly approaching the temperature of blood heat. Concentration of the vapor is desirable, provided a sufficiency of air for the proper æration of the blood is admitted. These details are, however, comparatively unimportant and will be managed by the physician to suit the peculiar circumstances of each particular case.

Art. IV.—Some of the Uses of Acetate of Potash. An extract from St. Louis Hospital Report, Ward No. XI. By James W. Clemens, M. D., Attending Physician.

There is nothing peculiar in the treatment of malarial diseases as adopted in the ward beyond what is used by every physician, except in the use of one medicine, and as its good results have now been thoroughly tested on a broad scale, the manner of using it will be detailed, so that others not so familiar with it may know when and how to use it and how far it is to be relied on. Quinine, of course, is always employed, and, like the well tried veteran, rarely does it fail to do the work expected from it. Cases, however, now and then occur in which it fails, but more rarely, I believe, when used in conjunction with acetate of potash. This latter remedy has seemed to me to be of wonderful service, not in cutting short the disease, but, apparently, in rendering the system more susceptible to the peculiar influence of quinine, and in almost every case of malaria received in the ward it is ordered with confidence that it will fulfill the expectations now formed of it. Dr. Golding Bird was the first to point out its efficacy as an eliminative, and from an experience of some years in its use, I can fully corroborate the opinion of that accomplished physician.

If used at all it should be in large doses, frequently repeated and largely diluted with water. In the ward it is usually given in half drachm doses, repeated every two hours, and diluted with five or six ounces of water. It may be given at any time, either during the pyrexial or apyrexial stage, and continued until the urine becomes not only free but light colored. The only conditions to its successful employment necessary to be observed are, that there should be no diarrhœa or irritability of the bowels, and that the urine should be dark or brandy colored. With any diarrhœa or irritability of the bowels, the medicine given in the above dose will act as a purgative instead of producing its special influence through the system before it finally passes away by the increased secretion of urine. It is useless to combine opiates with it, under such circumstances, with the view of checking the bowels, as in my experience the peculiar and beneficial influence of the acetate is prevented by any combination with opium, and in cases where there is diarrhœa, or any tendency to it, it is best not to use it all. It is also useless to give it where the urine is free and light colored, as under such circumstances the urine is simply rendered alkaline, without any perceptible effect being produced on the disease. Remembering these two conditions, then, the acetate may be given freely, and with confidence that, with rarely an exception, it will act well. In remittent it is scarcely less effective than in intermittent fever. In the ward it is the usual prescription, and always goes hand in hand with quinine. Often have I known cases with the fever running very high during the whole twenty-four hours, so continuous and uniform, indeed, that a remission could scarcely be said to exist; with this there would be violent headache, suffusion of the face and eyes, so as to make me dread giving quinine; but the potash mixture, with or without a mercurial cathartic as was indicated, would in a short time produce a positive remission, during which decided doses of quinine could be administered without fear, and in two or three days the patient would be ready to be discharged. I usually give quinine at any period of remittent fever, but I prefer to select a remission if I can, believing that its peculiar effects are more marked and permanent than at any other period. Given freely at this time, very many cases will need no other medicine and recover without further attendance. The action of the acetate results in the production of more or less positive remission—the very point I usually aim for—and in my hands has been very satisfactory.

Another condition in which I attach great value to the acetate is in old cases of intermittents. Every one in a malarious district has met with cases of repeated attacks of chill and fever, for which quinine has been given time after time with only temporary benefit. Every seventh, fourteenth, or twenty-first day the old enemy returns to be again subdued by its powerful adversary. This may continue for some time, until the patience of both practitioner and patient is well nigh exhausted. Arsenic or other remedies, probably, have been used with no avail, or have been deemed inexpedient. Under these circumstances, the patient may have enlarged liver or spleen or both; at all events the strength becomes reduced, appetite disappears, and the face begins to present that anæmic appearance following a continuance of malarial poison. In the majority of these cases which have fallen under my observation, the bowels have been constipated and the urine scanty and brandy colored. In these cases the potash salt is ordered and continued several days in succession, accompanied or not with small doses of blue mass and a cathartic, as may be thought best. The urine soon becomes abundant and light colored, and the face clearer and brighter; quinine is given in two grain doses for two or three days, with the effect usually of overcoming completely the oft-returning enemy. Here, as in remittent fever cited above, the acetate seems to prepare the way for the more effectual influence of quinine on the system, an influence which has been lost probably through some imperfection in the metamorphic changes by which the system is kept at par.

As to the mode of action of acetate of potash, I can only say that it acts as an eliminative. As to the special manner of its action—as to answering the question, how does an eliminative act?—we can give no answer. We do not know whether it is through the organic acid or the alkali that the good results follow—doubtless much depends on the combination of both. That it acts through those metamorphic changes by which disease is produced and cured, I have no doubt; but until these changes are better understood than at present, we can only surmise. Free dilution with water is essential to this action of the acetate. Without it, either no effect is apparent or the bowels are moved and the kidneys scarcely acted on; with it the urine rapidly increases in quantity, becomes lighter in color, and, if the remedy is continued long enough, alkaline. Water itself is a powerful diuretic, and should be given freely in all fevers. Experiments have proved that water will increase the metamorphosis of the body—judging from increased elimination—and if the increase is accompanied by an increase of food, the general strength and vigor of the individual will also be increased; a more rapid waste in this case being followed by a greater power of assimilation. Under the use of the acetate largely diluted, in such a disease as malarial fever, the tissues seem to be washed. Metamorphic changes rapidly go on, and the offending matter, in some form unknown, is either destroyed or thrown out of the body. Acetate of potash has had a variable reputation as a diuretic, and the reason is, doubtless, that it has been used indiscriminately, whereas it is only in special conditions that it will prove worthy of its ancient fame. Used in the manner and under the circumstances detailed above, it has rarely failed in my hands, and I can well believe it will prove as effectual in the hands of others. Another condition I must not forget to give: *Never* use it in typhoid fever, or in low grades of continued fever.—[*St. Louis Medical and Surgical Journal.*

CLINICAL RECORDS.

"Ex Principiis, nascitur probabilitas: ex factis, vero veritas."

Art. I.—Clinical Lecture. Delivered at the Philadelphia Hospital, by Alfred Stillé, M. D., Professor of the Theory and Practice of Medicine and of Clinical Medicine in the University of Pennsylvania, and one of the Physicians of the Hospital.

Lecture IV.—Emphysema of the Lungs and Chronic Bronchitis—Diagnosis from Phthisis-Treatment.

Gentlemen: I do not know that I shall have any great variety of cases to present to you this evening, but I have one case which, I think, offers some points of very peculiar interest. It is that of a woman whom I found in the ward appropriated to phthisical patients, and she has been considered as having that disease. I shall ask you to go over her history with me, observe the different points of the physical examination which I shall make, and then try to answer the question, has this patient had phthisis? The history which I took yesterday is to this effect:

Mary Devlin, æt. 42; born in Ireland; a domestic; was in this hospital five years ago, remaining two weeks, and was under treatment then for a cough, which she at first stated to be of five years' standing. Subsequently, however, it appeared that sixteen years ago she had a cough which she attributed to a cold. Before that time she never had shortness of breath or cough. During the early years of the cough she had frequent attacks of hæmoptysis, always small in quantity, and occurring during and after bad spells of coughing and not otherwise. Never was entirely rid of cough; it continued all the year round and was not, she thinks, aggravated by cold. Five years ago she had difficulty of respiration in going up stairs, and during the whole of the fifteen or sixteen years had more or less expectoration of sputa, which were sometimes white and frothy, and at other times yellowish, separated and floating in a watery liquid. Had frequent paroxysms of dyspnœa, always attended by coughing—never without it. These paroxysms are severe, but are relieved by expectoration. During the paroxysms she has violent palpitation of the heart; when not confined to bed has had œdema of the feet and legs. Appetite poor, with vomiting sometimes, provoked by coughing; bowels costive; menstruation irregular for several years; pulse small and weak, 84 beats per minute; pulse of left arm stronger than that of the right; no intermission, but the force unequal; face somewhat turgid; lips rather dark; apex of heart beating near the lower edge of the costal cartilages and faintly heard; in the normal præcordial region percussion resonance loud; heart sounds appear to be lower down than natural; maximum of basic sounds at the junction of the fifth rib with the sternum; maximum of apex sounds at the cartilages of the left false ribs; heart sounds feeble, especially at the apex, but all free of murmur.

On inspection, the movements of the chest appear as if it was in one piece; respiration wheezing. During inspiration the inspiratory muscles act powerfully and are strongly defined. There is no striking distension of the jugular veins. Expiration three or four times longer than inspiration, and accompanied with wheezing, cooing and other dry murmurs; no bubbling sounds audible. The treatment has been by copaiba internally and inhalations of tar water.

Here then, is a case that has been considered one of phthisis; but, probably, after you hear all that may be said about it, you will come to a different conclusion.

I wish, especially, to fix your attention upon the movements of the chest. You will notice, gentlemen, that there is a very decided movement of elevation, much more decided than the movement of expansion. You will, also, notice that every movement of the chest is accompanied by strong action of the muscles about the neck. These muscles, whenever the patient breathes deeply, are thrown into active contraction, so that you can see the depression between them very distinctly. You will also see that, at every inspiration, there is a depression behind the clavicle. The clavicle rises and there is formed a very deep hollow behind it. Now, these phenomena show that the chest does not expand as it should. The ordinary act of inspiration is accompanied with an expansion of the walls of the chest in all directions; the ribs are raised, and, at the same time, they expand laterally, and all the diameters of the thoracic cavity are increased. But, in this case, the only diameter that is increased is the vertical. Why is that? The reason plainly is, that the lungs do not expand much during the respiratory act. *Why* they do not expand we may speak of hereafter.

Listen, now, to the percussion sounds. You will notice that the resonance is very great on the anterior surface of the chest and under the clavicle, and even upon the clavicle. Even at the base of the lung (behind) there is also a very full, loud and resonant tone on percussion. You may also notice here what I have already called your attention to, that when the patient breathes, there is not so much lateral expansion as there is a vertical elevation of the ribs. There is one other point to show you before the patient goes. You will notice she has quite a good color; the lips are rather dark; the cheeks are apparently a little congested, and there is no pallor at all.

Now, there are several points in this case, gentlemen, which are of interest. The patient, as you have heard, has been regarded as having tuberculous phthisis. When I was so told, and found this patient in the wards devoted to the treatment of phthisical patients, I was struck at once with those features which I called your attention to a moment ago; that is to say, the face, instead of being pale, was rather colored, indeed, rather purple, and the lips were decidedly so. There was none of that appearance of anæmia which you so commonly find in phthisis; and, when I was told that she had been sick for a long time (for sixteen years), I began to suspect that, probably, some mistake had been made in the diagnosis; and, because such a mistake might be made by you, I brought this case before you in order that you might recognize the grounds upon which such an error was based, and in order that you would be prevented from falling into similar ones yourselves.

The history of the case, such as I have read to you, dates the disease back sixteen years. Now, there is no case on record of simple tuberculous phthisis lasting for sixteen years, with such symptoms as have been presented here. There are cases of chronic-phthisis running on for many years (cases running even a longer course than sixteen years), but they are all totally different from the one before us. They are cases in which there has been a commencement of the disease in the usual way; that is to say, with a short, dry, hacking cough, and a gradual and steady loss of flesh up to a certain point. Then, on examination of the chest, there has been found dullness on percussion under one of the clavicles, and, after a time, bronchial respiration, accompanied, perhaps, with gurgling, but confined to a limited space at the summit of one lung. This has gradually subsided and been followed by a rough or tubular respiration, limited to one point, the rest of the lung being perfectly unaffected. This quiescence has lasted for a year, or two, or three years, and then another development of tubercles in the lungs has taken place, either in the same part or in another part of the opposite lung. This is the usual course of chronic phthisis, but always with it, there is a steady deterioration of the health down to a certain point, and always in such cases there is a peculiar pallor of the countenance, such as there is no trace of, in the case before us. Almost always, too, in such cases, where there has been hæmoptysis, (and it usually occurs in them,) it does so in a manner peculiar to tuberculous disease of the lungs; that is to say, without any warning, without any apparent exciting cause, least of all, without any act of coughing at the time, the patient throws up from the lung a considerable quantity of pure, light-colored blood,—arterial blood,—and he throws it up in one, or two, or more mouthsful. The hæmorrhage of almost all cases of tuberculous-phthisis is of that character, occurring without any immediate exciting cause. But how is it in the case before us? Here we have had a cold, in the first instance, producing a cough which has continued ever since without any intermission—as bad in summer as in winter; at least, this is the statement given by the patient, in which statement, by-the-by, I presume she is a little in error; still, she is quite sure it continues the whole year round. This cough has been going on for sixteen years, and during the whole of this period there have been no periods of time when the patient could consider herself well; no period when there was a cessation of the cough; none when there was any great loss of flesh, and, therefore, there has not been in her case that succession of phenomena which you find in prolonged cases of chronic-phthisis.

But, then, you say she has had hæmoptysis. Yes, she has, but what kind of hæmoptysis was it? The hæmoptysis she has had has been of a different kind, from that I have just described to you, as being characteristic of chronic-phthisis. It has been produced by violent coughing; the blood has always been dark in color and always mixed with the sputa. Sometimes a larger portion of blood has come up unmixed with sputa; sometimes a very little was diffused throughout the sputa, but it always presented the characters I have mentioned; that is to say, it was dark in color, large in quantity, and generally mixed with the sputa. I repeat it, these are not the characters of hæmoptysis in tuberculous consumption. But, you will ask, what is the cause of this hæmoptysis? If she has no tubercles in her lungs, you will say, perhaps, she has disease of the heart. Very well, as it was quite probable that she might have, I examined the heart very carefully. What are the conditions of the heart which produce hæmoptysis? You know very well they are all obstructive diseases of the heart, and that first among them stands valvular diseases. Now, valvular disease of the heart is not a thing of to-day, it lasts a long time; and, therefore, if it continued long enough in this patient to produce hæmoptysis there ought to be physical signs of it. Now, instead of that, the sounds of the heart are perfectly normal; there is no apex murmur; there is no basic murmur. We must, therefore, consider that all the valves of the heart are perfect. But why, then, has this hæmoptysis occurred? It is not from tuberculous-phthisis, nor is it from valvular disease of the heart. Have we in this case, as it stands, an explanation of the spitting of blood? Let us consider. Why does hæmoptysis occur in cases of valvular disease of the heart? It is not simply because the heart is diseased, it is because the heart is diseased in a particular way, and it is because that particular way consists in there being a barrier obstructing the circulation of the blood. What must be the result of such an obstruction? Of course, if you dam up a stream in one point of the current, it will probably break out at some other point, and so it is here; if in the heart a barrier is opposed to the circulation of the blood, the blood, being dammed up, must tend to break out in some other part. But it is not necessary that the obstruction should be in the heart alone any more than an obstruction in the course of a river must be in one point only of its course; the dam can be erected anywhere and the current will overflow its banks. In the thoracic circulation you have a channel through which the blood is passing through the heart and lungs and to the system. If in any part of that you obstruct the blood it will tend to escape between that barrier and the point where the propelling force operates. In this particular case there is a barrier, not in the heart, but in the lungs. You have seen the signs by which we infer its existence. We have percussed this patient before you and you have noticed that the lung is permanently distended with air. What effect has that distention of the lungs upon the circulation? The lungs, you know, by their alternate contraction and expansion, aid in the circulation of the blood. But suppose the lungs do not act fully. Suppose for a moment that the lungs do not act at all. The blood must then accumulate in the lungs and in the heart, and, the heart continuing to act, the blood is dammed up between and in two opposing obstacles—the heart and the lungs. What follows? Something must give way. Evidently the blood must escape through some outlet, and the lungs being the readiest outlet, it does escape through them into the bronchia, and thence is impelled by coughing. Observe, also, that it is the venous system of blood-vessels in such cases that is the most engorged, and that is the reason why, in all such cases as this, the hæmorrhage is venous and not arterial. In phthisical hæmorrhage you have always arterial blood, and why? There are certain exceptions to this rule which I will not now allude to, except to say that they are very rare. Suppose that a vein runs along the side of a cavity, that vessel may be ruptured or eroded, and the blood thrown out will be venous. But that is not the actual source of hæmorrhage in phthisis. It is from the arteries that the blood is derived, because there is a change going on in the nutrition of the lungs which involves an alteration in the condition of the arteries, chiefly an increase, probably, in their diameters; and as the special lesion of consumption consists in the deposit of tuberculous matter by these arteries, the substitution of an unorganized aplastic material tends to weaken the walls of the arteries and later, the process of its softening tends to open those arteries, and the blood escapes from them. That is one reason why the hæmorrhage in phthisis should be arterial, while in a case like this before us, of mere mechanical obstruction to the pulmonary circulation, we have venous hæmorrhage, and, having venous hæmorrhage, we infer, with great probability, from that alone there is no phthisis, but that there is either heart disease or some obstructive lung disease different from phthisis. But we have shown that it is not heart disease. If, then, there is lung disease, what is it? Consider, first, the long continuance of this affection, to which I have already called your attention as being inconsistent with phthisis. What does that imply? Simply, that the disease is chronic-bronchitis and nothing else. But chronic-bronchitis has certain peculiarities of expectoration. Have we them here? I cannot now show you the expectoration which this patient has thrown up, but it is quite characteristic of certain forms of chronic-bronchitis, that is to say, it is very copious, and it consists of a liquid which is more or less transparent, in which float greenish or yellow colored masses, which do not run together; that is, each sputum remains more or less distinct. There is no other disease in which such sputa appear. In phthisis the sputa do not present these colors, unless exceptionally and temporarily. Here they are constant and permanent.

In other cases of chronic-bronchitis, and in some advanced cases of phthisis, the expectoration alters very much; it becomes purulent instead of mucous; the sputa run together; they form a homogeneous liquid, as all pus does. In this case there is no evidence of pus at all; but there is a great deal of this sputa, such as I have described—a pint and a half, at least, during the day. These sputa, then, are not the sputa of phthisis, but of chronic-bronchitis. They have other qualities, also, which are very frequently met with in the later disease. They have a nauseous and peculiarly fetid odor, which is different from that which you usually meet with in phthisis. It is sometimes observed in the latter disease in a greater or less degree, but only in very rare and exceptional cases in any other disease than chronic-bronchitis. I have seen cases of even rapid phthisis where the expectoration was intensely fetid. I have in my mind such a case, where the odor was so horrible that it was insufferable, not only in the room where the patient lay, but it even filled the whole house. In the chronic-bronchitis we have before us there is a different kind of odor—a nauseous and a fetid odor at the same time.

There is another point which I want to call your attention to, since you have seen this patient breathe before you. If you will recall any case of advanced phthisis you have seen, you will remember that the breathing is always hurried, rapid, panting sometimes, and particularly during the periods of fever; but you have noticed that the respiration of the patient was not hurried, but extremely labored. Why was it labored? I have given you the reason in anticipation. We will, however, return to it again.

In the case before us, then, there is not the expectoration of phthisis; there is not the characteristic respiration; and then, again, the loss of flesh, which is an unfailing symptom in chronic-phthisis, is not marked here. There is no pallor, no pinched nostrils, no thin lips, no hectic flush of the cheeks. On the contrary, we have here a face rather over-colored, with red cheeks and rather turgid lips. These show merely, not that there is too much blood, but that that blood cannot circulate freely.

This patient has stated that she was formerly quite stout, and that at some period of her disease she lost flesh rapidly. You will say this was an indication of phthisis. Very true, it is so; but there is a qualification to the statement which at once destroys the force of the analogy. In phthisis the loss of flesh is steadily progressive when once it begins; unless it is arrested by appropriate treatment, it goes on steadily to the end. In chronic-bronchitis you do not have that steady progression downwards. The patient eats but very little; the cough destroys the appetite for food, and in this way, partly, the flesh is lost. In addition to that you will remember the enormous quantity of sputa that this patient, for example, rejects· Here is a loss of highly animalized matter, perhaps equal to the whole amount of food she takes in as nourishment during the day, so, of course, she must lose flesh; but she does not lose flesh beyond a certain degree, whereas in phthisis she would go on, and on, and on losing flesh, and, if life held out long enough, she would have nothing left but the skin which covers her bones.

Now, let us look at the physical signs which we have noticed here. You know where tubercle is usually deposited in the lungs, and in chronic-phthisis it is almost always deposited at the apex of one or the other lung, and that its deposition there gives rise to dullness on percussion.

This is the unfailing accompaniment of phthisis in ninety-nine cases out of a hundred, and with it you have a depression above one of the clavicles, and you have decreased mobility of the chest at that point. But what do you notice here? Dullness below either clavicle? On the contrary, you have increased resonance under and upon both clavicles, and not only there, but throughout the whole chest, you have exaggerated resonance. Here, then, you have not the physical signs of phthisis in the least degree, for you have no percussion-dullness anywhere. But there is another fact revealed by percussion which is of extreme importance. When we came to look for the heart in its usual seat we did not find it. You know that you should find dullness beginning about the left third rib and becoming more positive as you go downwards. At the third rib, and below that, when we percussed the patient, even below the fifth rib, there was no dullness. What, then, had become of the heart? On percussing a little lower down there was dullness. Going down again below the nipple—below the mammæ entirely—we at last came to the apex of the heart as revealed by percussion, and also by sight. Consequently, the whole heart has been depressed from its normal position. Why? Is it depressed in cases of phthisis? Never. On the contrary, if there is any considerable diminution in the capacity of the lungs, the heart is a little raised, if anything, but here it is very much depressed, and why? We have found, on percussion, a large increase of vibration in the walls of the chest, showing that there is a great quantity of air within them. The lungs, then, have become distended, and, of course, they have pushed before them whatever would move—the liver, the aorta, and the heart.

We have still another point on which to add a word of explanation—the hæmoptysis. You remember what I said about it. I will now add that this dislocation of the heart has contributed, also, to produce it. Of course, pushing the heart out of its position implies disturbing its proper relations with the aorta; it implies an alteration in the position of all the great vessels. In other words, the natural direction of those channels has been altered, which could not be without impeding the flow of the blood through them. In consequence of this impediment there has been a great difficulty in the passage of the blood from the heart to the lungs, and in it we must recognize an additional cause for the hæmorrhage.

You have heard, some of you, the peculiarities of the respiration in this patient; that is to say, you have heard that the inspiration is very short and the expiration very long. Let us account for that. Inspiration, as you know, is generally longer than expiration, because the lungs dilate very fully; they move over a large space, and during expiration the return movement over the same space is rapid. Inspiration, you will remember, is a force acting all the time against an obstacle; expiration is a force acting without an obstacle. But, here, in inspiration you have a very short sound; it is, perhaps, not more than one-fourth and from that to one-third the duration of expiration. What is the reason? Why, the lungs are already almost fully expanded by the emphysematous disease. How then can they expand farther? Only by a greater effort, on the part of the patient, can any air at all be drawn into the lungs. You saw this great effort; you saw that the muscles of the neck were in active and violent exertion, showing there was a prodigious obstacle to overcome, a great weight to lift. But this, necessarily, was a short effort, the lungs, being already filled with air nearly to their full capacity, could not receive much more, so that, when you listened, you heard but a very short inspiratory murmur, and that was unaccompanied by any marked sound, except by the natural one of the air passing into the lungs—a little rough, a little dryer than natural, nothing more. But, as soon as the patient began to expire, the breathing sounds were changed; you began at once to hear loud sounds, cooing and whistling, and, to a certain extent, crackling all over the lungs wherever you listened. What was the reason of this? Why, all the muscles of expiration were trying to force out the foul air in the lungs; they kept up that effort for three or four times as long as the time employed in inspiration, and in doing so the air became engaged with the mucus which chokes up the smaller tubes and produced the various sounds which you heard.

Here we have, then, an explanation of the cause of the difference in time between the inspiration and expiration, and an explanation of the respiratory murmurs. Now, why are there these rhonchi of different characters? You have heard me describe this sputa which this patient expectorates; it comes from the whole surface of the bronchial tree. In the larger bronchi it gives rise to no certain phenomena except when the secretion has been accumulating for a number of hours. If it has been expectorated freely, you then hear nothing in the larger bronchi; it is only in the smaller bronchi that the sounds are generated. But why? What causes this enormous secretion? Why, there is an inflammation of the mucous membrane lining the whole of those bronchi, and that inflammation results in the throwing off the different liquids before described—of the mucus, the pus, the serum, and the blood occasionally. But this prolonged inflammation, which has lasted in the present case for sixteen years, has done more than this: it has produced certain physical changes in the structure of the bronchial tubes, and it always does. Examine post mortem the lungs of a patient who had had chronic-bronchitis and you find that the mucous membrane has become thickened, and instead of a pinkish hue it has a dark color, and when you incise the bronchi you at once observe how hard they are; how they grate between the blades of the scissors. And why is this? Because they have been converted into a thick, fibrous, almost cartilaginous substance. Now, what effect has this upon the smaller tubes? Evidently as the tube walls thicken their calibre diminishes. In some cases of this affection the membrane becomes so thick that the walls of the tubes come together so that no air at all can enter that portion of the lung with which they communicate. These are the anatomical changes which usually take place in the lungs.

There are other points which I might speak of in connection with this particular case, but I think I have said enough to lead you to agree with me entirely that the case before us is one, not of tuberculous phthisis, but of chronic-bronchitis, and that you have well illustrated in the particular phenomena of this case nearly all the prominent phenomena of that disease.

In regard to the treatment of this case, I may say that the principle of the treatment should be just that which you would apply in the treatment of any chronic inflammation where local applications can be made, we will say of the conjuctivæ of the nasal passages, of the throat, of the urethra, there is no essential difference. To treat these different diseases which I have mentioned, external diseases, all of them, and occupying accessible membranes, you are not in the habit of giving medicines by the stomach, except as auxiliaries. Why, then, has it been the practice to administer them here? Simply for the reason that, until very lately, we have had no means of introducing medicines into the air passages to modify the condition of the lining membrane. But now we have such means. I exhibited to you the other day an atomizer with which, by the help of steam, you can form a spray, and by which you can throw medicated vapors of all sorts into the air passages, and in doing so you can treat the lungs directly, as we have always treated the eyes, the nasal passages and the throat. I will not spend time by enumerating all the medicines you can use in this way; all which are usually administered by the stomach, intended to exercise a local influence on the lungs, can be given by the atomizer, and, in addition, astringents may be usefully employed in this way, although they are of little value in this disease when administered by the stomach.

It is hardly necessary for me to enumerate now all of the medicines which can be employed by inhalation, nor all of the particular ones to be used here. You know them already, for they were mentioned to you the other day. Creasote is one of the best. All the preparations of tar, all the terebinthinate preparations, even the common oil of turpentine, if you can get nothing better, will answer the purpose. There is, in most of these terebinthinate preparations, one peculiarity. It is this: that, when given by the stomach even, they have a special direction to all the mucous membranes and are eliminated by them. You know when a patient takes turpentine the breath smells of it; the urine, even the discharges of wind from the bowels, have a terebinthinate odor. They are all exhaled by the integument internally as well as externally, for you know the mucous membrane is only a modified form of the skin. The best of all the terebinthinates, and the one which is most efficient for internal administration, is copaiba. It has long been employed for chronic-bronchitis, and I doubt if there is any remedy that will compare with it in efficacy. Next to this will come perhaps medical naphtha, and the other preparations of tar. Benzoin is also very useful. There was a medicine, the composition of which was a long time kept secret, known as Turlington's balsam, which was considered, and I believe is still held by people who always put great faith in a remedy because it is a secret, to be very efficient, in which the chief ingredient is benzotn. All these may be given by the stomach with a firm conviction that they will come out by the mucous membrane, and in such a way that they will diminish the discharge, and by diminishing the discharge they will diminish the cause which gives rise to that discharge, and in doing that they will, of course, diminish the cough.

There are some remedies which have been employed in cases of this kind where the thickening of the bronchial mucous membrane may be affirmed to be considerable, and which should not be lost sight of. The most important of them all is muriate of ammonia. You may add this to the usual expectorant medicines which are given in such cases, and give it along with seneka, squill, &c. The muriate of ammonia is supposed, however, to promote the absorption of the interstitial matter which constitutes the cause of the thickening of the bronchial tubes. My own belief is that it is very efficient in this respect, but I do not give it usually until the amount of secretion from the mucous membrane has been very considerably lessened by the use of terebinthinate balsamic preparations.

ORIGINAL CORRESPONDENCE.

"Sit mihi Fas scribere audita."

VIENNA, February 8, 1868.

Since I left Paris I have traveled so rapidly and have found so many sights to see, that I have not devoted a very large amount of time to medical observations, though I have visited the hospitals in all the cities in which I have tarried in the hope of picking up something interesting to my professional brethern. I regret to say that I have found but little to commend in the hospitals of Italy, and, from what I have gathered from medical men resident in that country, I am convinced that our profession is no exception to the general decay of Italian institutions. The people are indolent and behind the times, and the doctors are like their patients. The hospitals in Naples have brick floors, and are dark, dirty, crowded and wretchedly ventilated. It is a wonder to me that the plague is not generated in them. The vile odors which fill them are enough to make well people sick, and to kill the invalids outright. Malarial affections, diseases the result of insufficient food, itch and prurigo from lice, comprise the majority of the complaints to be found here. I was in hopes of finding cases of leprosy, and impressed upon the stupid hospital steward, who conducted me through the wards, my wishes. To my utter astonishment and unbounded amusement, the fellow assured me that he regretted very much that I had not come to Naples sooner, since only a short time before they had five splendid cases of leprosy, but all had been cured and discharged! I laughed outright in his face, remarking to him that he must be mistaken, for leprosy was an extremely rare disease and entirely incurable. Before I left the hospital the steward seemed to have forgotten his first lie and told me that if I had come but a day or two sooner I could have seen one of the cases I was looking for; unfortunately, however, the patient had died the night before, and his was the only case of leprosy they had had for a long time.

In Rome it was my good fortune to meet two bright doctors, who were very kind in showing me the hospitals, and giving me much medical information pertaining to the Eternal City. Both are foreigners. Dr. Gould, formerly of the United States Navy, and now physician to the American Embassy, has practiced in Rome for the last nine years. Dr. Jaussig is a German, and like all German doctors, is cultivated and thoroughly informed in his profession. Any Americans who may have the misfortune to be sick in Rome may feel assured of careful attention and skillful treatment if they fall into the hands of either of these gentlemen.

Both Dr. Gould and Dr. Jaussig discredit the statement often made, and which is to be found in all the guide books, that the deaths in Rome exceed the births. Murray, for example, states that for the last ten years the births have averaged annually 5606, while the deaths have reached 6139, and that the slight growth of the city is due solely to foreign immigration. It is to be remarked of these medical gentlemen that they practice only among the visitors at Rome, or occasionally among the Roman nobility, and are comparatively but little acquainted with the condition of the masses of the people. Besides, in forming a judgment in the case, it is to be taken into account, that people are almost always blind to the drawbacks upon their homes, and very few think the city where they live unhealthy. All admit that beyond the city wall it is insalubrious, and during the summer no one stays in Rome who can get away. Surrounding the city are thousands upon thousands of acres of pasture land, some portions undulating, but most of it flat and marshy. This is the famous Campagna, which is the property of the Church and of wealthy nobles. Scattered over it are ruins of ancient temples, palaces, tombs, aqueducts, etc.; but it is almost uninhabited. Here and there is a shepherd's hut, with a little garden and a small grove of olive trees around it; but, except for these, it is a verdant, uncultivated wilderness. The vast flocks of sheep which feed upon it yield a large income to its proprietors. I asked why this great tract of apparently fertile soil was allowed to remain uncultivated? The reply was, that it was partly because the wealthy owners are contented with the income that it now yields; partly because the people are too indolent to work it, and partly because of the impression that turning up the soil by the plow breeds terrible fevers which kill those engaged in its cultivation.

In St. Paul's, and some of the other churches outside the city walls, malarial fevers are so prevalent among the priests in summer and autumn that it becomes necessary to relieve them constantly by relays from the city; and sometimes, it is said, these churches have to be temporarily closed on account of the severity of the fevers. The country people and the town orders of the citizens seen in Rome and Naples look sallow, peaked and chilly. Congestive chills are frequent and fatal in this neighborhood, and remittent and intermittent fevers, especially the latter, are common. Rheumatic affections and pneunomia prevail in winter, and heart diseases are said to be of exceedingly frequent occurrence. Bleeding and the antiphlogistic treatment is that which is generally adopted by the native physicians in these affections.

The hospitals of Rome are by no means creditable to the imperial city, but, on the contrary, except in a new ward in one of them where the Pope's Zouaves are cared for, are dark, cold, cheerless, and often dirty. All have brick floors, and, from the windows being placed at the tops of the walls, the wards have a prison-like air. In the hospital for skin-diseases, where there are several hundred cases, I saw one patient laboring under elephantiasis arabica—a striking and horrible sight; a case of pemphigus, and several ecthymas. The latter disease is quite frequent, I am told itch constituted a vast majority of the cases in the hospital. Syphilis also abounds. From the numberless cases of scabies and prurigo in the hospitals, and the amount of scratching which I have seen on the streets, I think it would be safe to say that Italy has the itch and is lousy.

In one of the hospital museums I saw the stuffed bust of a negro man, who came with Garibaldi to Rome in 1849, and was killed here, with the bust of a negro girl and of a white woman, both stuffed, as we stuff the skins of birds for preservation. They looked like wax figures, with glass eyes and lips painted. The appearance of the negroes was quite natural, and coloring, I think, had been put on their skins since they were stuffed.

In Florence the hospitals are superior to those just mentioned, but they are far from being patterns in any respects. In them I saw a number of cases of favus and several of pelagra, but none of the latter striking. Some beautiful wax preparations are to be seen in the hospital museum and some interesting morbid specimens. I saw there also a life-size wax figure of an old man who died some years since in Florence with the Arabian form of leprosy, and a wax mold of the genital organs of a hermaphrodite. There are, in fact, besides this, several other specimens of the latter deformity, but it is the most remarkable case I have ever seen. The subject is still living in Florence, an idiot, and if I could have learned his residence I should have gone to see him. The cast was taken when he was, apparently, about ten or twelve years of age. In the usual position is a penis about an inch or a little more, long. The glans composes half the length of the member, and is as large as the end of a man's finger, of the normal shape and color. Upon first view I thought it contained the meatus urinarius, but on a closer examination I discerned that there was no urethral aperture. Behind the glans the skin is gathered into loose folds. Just below this imperfect penis begin very well formed labia interna, of the usual size and color, and outside of these, commencing from the sides of the penis, are the labia externa, well formed and of the ordinary dimensions. At the inferior portion of the labia interna is an inflamed looking orifice, about the size of a goose quill, which is evidently the urethra. In Rome, Florence and Naples in the collections of ancient statuary, hermaphrodites frequently occur, and the subjects are invariably represented with a well formed penis and testicles, while the rest of the form and features, including mammæ, are entirely feminine. Among the art specimens in the Florence collection are several acephalous and almost amorphous fœtuses, and others curiously hideous.

In Venice the hospitals are like those in other parts of Italy, or, it may be, a trifle cleaner. I met with nothing of special interest in them. In the female lunatic asylum adjoining one of the hospitals I saw some cases of pelagra among the wretched inmates, not, however, well marked in feature. The lunatic asylum is a horrible place. It is very gloomy, and the poor crazy people were suffering from cold.

Vienna, as is well known, is one of the chief seats of medical learning in Europe, and there are now, it is said, more than a thousand students of medicine here, including fifty or sixty from America. I have had the pleasure of meeting amongst these Dr. Geddings, of Charleston, Dr. Shepherd, of New Orleans, and Dr. Jenks, of Philadelphia, cultivated, affable gentlemen, who are making the most of the rare facilities afforded by this capital for perfecting themselves in their profession.

The medical magnates of Vienna look less like great men than the eminent doctors of France and Great Britain do, and these, as I have remarked elsewhere, give in very rare instances any outward signs of greatness. Rokitansky is of excessively ordinary appearance. He is of medium size, of a muddy complexion, with dull gray eyes, coarse features, short, gray side whiskers, and was wearing, when I saw him, a worn and shabby coat, a greasy black silk stock and no shirt collar. His whole dress was slovenly, and even dirty. He makes one think of the great Suwarrow, who eschewed the effeminate luxury of soap and water, and always slept in his clothes. Dr. Rokitansky goes through the form of giving a course of lectures each season, but he speaks so carelessly and indistinctly that even German students are unable to understand him, and very few attend his lectures. His collection of pathological specimens is by no means so large as many others in Europe, but it has been made with so much judgment that it is said to be first in the world. His collections of deformed pelves, abnormal genital organs of the female, and diseased hearts, are considered the most valuable portions of his museum. In each of these classes every deformity and every morbid change which has probably ever occurred in these organs is represented.

Hebra is a short, round man, with a round head, and round, fat face on a short, thick neck. He has a big, round belly, and chubby, round hands. Over a pair of big round eyes, of a pale blue color, he wears a pair of spectacles; and below a short pug nose he wears a whitish mustache of moderate dimensions. A black slouch hat covers his short-cropped flaxen hair, and his coat snd pantaloons are of a coarse speckled material. He looks like one of those coarse beer venders whom you see at the Dutch gardens of our cities. He is said to be excessively penurious, and, as a man, is not popular, but as a teacher he commands respect. The medical professors here are allowed to have private classes, and, although the charge of Hebra is more than double that of any other teacher, he always has many private pupils. He charges a fee for every successive course, no matter how often you may have listened to him, and one gentleman was pointed out to me who, it was said, had just paid him the fee for his *ninth* course. He maintains a private hospital, which he makes very profitable. His charges are enormous, and he concedes nothing even to his sovereign. When the Austrian princess met with the dreadful accident last spring, from which she finally died, (your readers will remember that her clothes caught fire and she was fatally burned,) Hebra was called to attend her. He refused to do so unless the princess was brought to his hospital, alleging that he could not treat her properly elsewhere. He had all the necessary appliances at his hospital, he said, and at the palace could not command them, and was unwilling to assume the responsibility of such a case without every facility of curing his patient. The Emperor refused to yield to his demands and another medical attendant, but of less note, was called in. This incident may show at once Hebra's importance and the diminished authority of Austrian kings.

One object that I had in visiting Vienna was to see a case of *Plica Polonica*, and, to my great surprise, I find that Hebra denies the existence of any such disease! He affirms that the matting of the hair and the exudations which cover it are due to syphilis, or to some other disease of the scalp, and that the affection is not in the hair itself.

With my talented and obliging friend, Dr. Jenks, I have attended the Clinique of Oppölzer on general diseases, that of Azll on the eye, Widerhofer's on children, Sigmund's on syphilis, and Braun's obstetrical clinique, besides that of Hebra on skin diseases. Rokitansky I saw talking over a cadaver at a post mortem examination.

The majority of the children at Widerhofer's clinique had constitutional syphilis. This disease is said to be more prevalent here than in Paris, and pervades all classes of society. Calomel in doses of the sixth of a grain and the corrosive sublimate bath form Widerhofer's treatment for syphilitic children. He dissolves ten grains of corrosive sublimate in a bath of ordinary size. I should think it a good treatment.

Ricord's views in regard to constitutional syphilis are those which obtain in Vienna. The hard chancre, the physicians here believe, is the fountain of the disorder.

In the Lying-in hospital of Vienna, the births average twenty-five a day, and a short time since thirty-six infants came into the world, within its walls, in four and twenty hours. Every facility is afforded the student here for studying obstetrics, and during his stay he is almost sure to see every variety of presentations and every accident that occurs to lying-in women. While I was in the lying-in chamber there were three women in labor, and Braun delivered one of them of a breech presentation before the class with instruments.

Medical teaching in Vienna is confined almost exclusively to one huge hospital, in which there are over three thousand beds. The lectures and cliniques succeed each other in the different wards and lecture rooms, so that a student may occupy himself from 7 o'clock in the morning till 9 at night in this institution, studying the different sections of medicine. The post mortem examinations amount to twenty or more a day.

Of all the European cities which I have visited Vienna affords by far the greatest opportunities for learning medicine; but I am more and more convinced, by all I see, that America is the place where it should be studied by Americans. In these old cities, it is true, larger numbers of cases are shown, and the material in the hospitals is more completely at the disposal of teachers and students than in our country; and, for one who wishes to study obstetrics, or skin diseases, or, in fact, any other specialty, or for a doctor who desires to rub himself up and see vast numbers of cases in a short time, it is unquestionably well to come to Vienna. But to the beginner, I am sure, there is no place like home. There is more system, more common sense, less old fogyism in our medical schools than in those anywhere in Europe. Already the world admits that American dentists are superior to all others. In surgery and obstetrics we excel the Europeans. Gross' surgery is translated into German and Dutch, and, I believe, into French, and has been reprinted in England. No authority in surgery is oftener appealed to in those countries. Some one has predicted that in fifty years Europeans will be flocking to America to study medicine, and I believe it. Europe travels in an ox-cart; we go by steam. Europe has the stumbling blocks and obstacles of ages of prejudice to retard her progress; we have a smooth track and new machinery.

All the way from the Austrian frontier to Vienna I found the earth covered by snow, often a foot, and sometimes two feet deep. Every day in Vienna we have had a little snow, but soon followed by sunshine and warmth. The temperature varies several times daily, but it has not been very cold at any time. Consumption, I learn, is the scourge of Vienna. Prof. Oppölzer speaks of it, in his lectures, not as phthisis, but as "the disease of Vienna."

Respectfully, &c.,

L. P. YANDELL, JR.

NEW CANTON, BUCKINGHAM, May, 1868.

In 1858, when there was not as much known about dipththeria as now, among other personal observations tending to prove the contagiousness of that disease, I witnessed the following:

A little child at the breast had diphtheria, but continued to nurse. In a few days, the mother complained of sore nipples. On examination I found both of them *covered with the peculiar diphtherilic membrane,* which extended over the mammæ to the distance of inch or more.

At the same time a little girl, the child's sister, who, by the way, had assisted in tending the infant, feeding and fondling it, took the disease. This was an unusually bad case. The pharynx and nares were completely lined with membranous exudations. She fell into the habit of picking at her nostrils, until she picked a hole through the septum. In this exercise she used the thumb and next two fingers of the right hand until they too *became sore,* and were found to be invested with the same membrane as that in her throat and nostrils.

The mother whose breast exhibited this phenomenon had no sore throat, or any other *local* manifestation of the disease, but suffered all the constitutional disturbances generally attending this formidable distemper.

Those were the plainest and strongest proofs I have ever *seen* of the propagation of diphtheria by *contact,* and prepared me to believe many similar reports on good authority, as, for example, the development of diphtheritic membrane between the toes of nurses who had stood barefooted on excretions from the throats and mouths of diphtheria patients, &c. F.

CAMDEN, S. C., April 13, 1868.

In the last (April) number of your valuable Journal, you state with reference to the decoloring agency of carbolic acid on iodine, that "several pharmaceutists, entirely competent, have made a trial of the formula (given by Dr. Percy Boulton), and there has been no decoloration whatever."

I desire to call your attention to the fact that, in a letter to the editor of the New York Medical Record (No. of February 15th, 1868), I announced the discovery of an efficient decoloring agent of iodine, which, I am confident, will not disappoint any pharmaceutist. I quote from the above mentioned letter:

"I have ascertained that the hyposulphite of soda has the peculiar effect of depriving iodine of its color, forming a perfectly limpid fluid, which does not form the purple iodide of starch on the linen, nor produce the yellow discoloration of the skin. The hyposulphite having no medicinal effect in the small quantity required for this purpose, the tincture of iodine suffers no addition to, nor detraction from its therapeutic properties.

"I make a saturated solution of the hyposulphite in water, and this is added in proportion of about one-sixth of the tincture of iodine. The latter floats upon the former, but through agitation will produce a clean solution with the properties mentioned. A solution of the bisulphite answers the same purpose.

"If we desire to obtain the effect of the undiluted tincture iodine, we need only dissolve a few crystals of the hyposulphite, or a little of powder of the bisulphite in the tincture, and complete decoloration will be the result."

I would add that, in the latter experiment, a few drops of water poured on the sulphites will insure rapidity of effect.

Yours, very respectfully,

S. Baruch, M. D.

City Hospital, St. Louis, Mo., April 16, 1868.

If any of your surgeons would like to use my splint,* I think it would be to their advantage to send their orders here, as I can have them manufactured cheaper than they can, while I would see myself that the apparatus is properly adjusted for use before it is packed for shipping. They can be made here complete for $18.

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*Illustrated in this Journal.

I am now maturing three more new apparatus for treating fractures of the femur, fractures about the upper end of the humerus and for fractures of the olecranon, and will have them ready for publication next month.

I omitted to state in the article that I allow my patients to go on crutches immediately, when they are not otherwise disabled, and can bear half the weight of the body on the injured leg, without any danger of modifying the force of the extension.

E. A. CLARK, M. D.

GREENSBORO', GA., February 19, 1868.

Agreeably to previous notice, a large majority of the practising physicians of Greene county met in Greenesboro' this day, when, on motion, Dr. John E. Walker was called to the chair, and Dr. A. A. Jernigan was requested to act as secretary.

On motion, the following gentlemen were appointed a committee to report business for the meeting, to wit: Drs. W. A. Moore, W. H. Credille, G. W. Durham, W. L. Bethea, L. W. Kimbrough, H. H. King and A. A. Jernigan.

The committee reported a fee bill and excellent resolutions, which were unanimously adopoted.

The following gentlemen subscribed to the fee bill, a copy of which can be procured from any one of the committee:

JOHN E. WALKER, M. D.
H. H. KING, M. D.
W. A. MOORE, M. D.
A. H. RANDLE, M. D.
A. A. JERNIGAN, M. D.
I. D. MOORE, M. D.
WM. L. M. HARRIS, M. D.
W. H. CREDILLE, M. D.
THOMAS P. JANES, M. D.
L. M. KIMBROUGH, M. D.
T. W. LANDRUM, M. D.
JAMES M. GRIFFIN, M. D.
GEO. W. DURHAM, M. D.
W. L. BETHEA, M. D.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

"Judex Damnatur cum nocens Absolvitur."

Atlas of Venereal Diseases. By A. Cullerier, Surgeon to the Hôpital du Midi, ect. Translated from the French, with Notes and Additions, by Freeman J. Bumstead, M. D., Professor of Venereal Diseases in the College of Physicians and Surgeons, New York. Philadelphia: Henry C. Lea, 1868. 4to, pp. 140. With three Chromo-Lithographic Plates.

Of making many books—on the venereal disease, at least—there is no end. Less than a year ago, Mr. S. Key, Spencer Smith, and five other clever Englishmen, concluded their long-looked for Report on Syphilis. It was published by the British Government. It occupied nearly six hundred pages royal octavo. It embodied the views of sixty-four experts, among whom are found the names of Erasmus Wilson, Langston Parker, and others of the best men in Great Britain. It was received as the deliverance of the British school; and, in the cloud of witnesses whose testimony it contained, it was thought to reflect the opinions of most other schools besides. We supposed syphilis was done for—at least for awhile. We were mistaken. We had scarcely finished the Report, when it was announced that Dr. Bumstead intended to translate and edit the work of M. Cullerier on venereal diseases.

The first part of this labor is before us. The introductory chapter is occupied, with the history, contagion, virulence, evolution, inheritance, and pathological anatomy of syphilis. The second chapter is devoted to blennorrhagia.

M. Cullevier is a unitist. His translator is equally well known as a dualist. Both author and translator appear in the introduction to great advantage. The views of both are excellently rendered. Bumstead's notes are in his best style. They are incisive, but admirable in spirit, wonderfully strong both in logic and fact, and not too copious. Our present purpose, however, is simply to call attention to the work. It is to be completed in five parts, and they are promised in quick succession.

In the May number of this Journal, the editorial exhibits certain views in regard to the "editing and translating of foreign works," which we fully endorse. They do not, however, apply to Dr. Bumstead, who is himself an authority—confessedly the highest in this country on this class of diseases, and the author of the best book, in our judgment, on the venereal in any language. Few men can be better fitted than he, both by experience and study, to edit such a work, but our wonder is that he should have undertaken the drudgery of a translation.

The publisher, Mr. Lea, has placed the profession under renewed obligation by the manner in which he has done his part of the work. The letter press is perfect. The plates are done in a very high style of chromo-lithography, and are splendidly executed. A cotemporary, who has had an opportunity of personal examination, says they will compare favorably in distinctness and brilliancy with the originals.

D. W. Y.

RHEUMATISM. By HENRY WILLIAM FULLER, M. D., Cantab, Fellow of the Royal College of Physicians, etc. Philadelphia: Lindsay & Blakiston,, 1864.

The highest authority on the subject of rheumatism, especially its etiology, is unquestionably Henry William Fuller, of London.

Can this statement be substantiated? We think it can.

First—His position as registrar and physician of St. George's Hospital for years gives him high repute.

Second—As a fellow of the Royal College of Physicians and Surgeons, he advances farther as an authority.

Third—The very valuable tabular statistics that he has made carries him still farther.

It may be said that the first reason can be made for and is enjoyed by many others. It is admitted. That the second is also the possession of many others. It is admitted. That the third may or may not be of value, inasmuch as statistics are sometimes used as the "means to establish theory despite the truth," which is also admitted. This, then, brings up the fourth and last test, in which test his statistics may be placed, and, if not proven true, we allow his claim to be nil.

Now all discoverers, with few exceptions, have had competitors, who, with more or less plausibility, contested the right of priority.

Virchow, Bennett; Bouillaud, Watson, Stokes; Duguerre, Talbot; Humphrey Davy, Volta, are a few of the names of the prominent instances of the fact as given in Leucocythamia—the *to* and *fro* sound of pericarditis, photography, etc.

Says the London Quarterly Review: "It is within our recollection that when some one made a remark in his presence as to the value of his discoveries in the decomposition of the alkalies and earths, Sir Humphrey Davy observed, 'Perhaps you give me more credit than really belongs me; others had invented the voltaic battery. The time had arrived when it was to be applied to the purposes of chemistry, and it fell first into my hands.' Something like this may be said as to most of the great discoveries which have been made in the department of physical science. Knowledge is, for the most part, slow in its progress. Among those who are engaged in the pursuit of it, there are few who do not add something, however small the contribution may be, to the general stock. At last some one endowed with a more comprehensive genius, taking advantage of the labors of his predecessors, views the facts which they have overlooked, and from thence is led on to further inquiries, which open up new views of natural phenomena and afford a deeper insight into the laws by which they are regulated."

Fuller's claims are fully up to all the ordinary examples in the line of discovery—faintly foreshadowed by others it remained for him to reduce to rule, to law, to science.

We quote from a very eminent medical author: "The science of medicine is especially amenable to the rules of inductive reasoning. Having to unravel the mysterious phenomena of life and to investigate and treat those deviations from its normal state termed disease, the physician is peculiarly exposed to many sources of error and fallacy unless he cautiously keeps in view the great truths inculcated by the Baconian philosophy. It is only by applying the principles of the inductive process of reasoning by which we conclude that what is true of certain individuals of a class is true of the whole class; or that which is true at certain times will be true under similar circumstances at all times, that medicine will take rank with the exact sciences, and its cultivators have a right to claim a foremost position among the distinguished philosophers of the day. In the study of medicine, perhaps more than in any other science, we are peculiarly exposed to the danger of adopting false facts, of being seduced by specious and hasty generalizations and led into error, deducing general principles from the consideration of a *few* particulars—the bane of all right and sound reasoning, the foundation of all bad philosophy."

We then find Fuller firmly planted in the inductive philosophy, which is our fourth reason. But is it true that he is so planted?

His proposition is that rheumatism is dependent upon the presence of a poison—blood poison—which poison is an acid. Lactic acid?

The requisition of the inductive process is the positive proof of the presence of acid in excess in the blood and the secretions in each and every case of rheumatism, knowing that if it should be shown that an excess of acid is not found in a single case of true rheumatism, the theory falls to the ground. He has shown the presence of acid in every case. He has never known an exception. Having made the assertion, no one has reported an exception. The challenge has been virtually before the world since 1845. He has the endorsement of his eminent colaborer, Garod. We accept the inevitable conclusion that the claim is established—the production of an excess of acid is essential to the production of rheumatism. So far as human reason can arrive at certainty he has exhausted her power.

Shall we turn to the treatment? Does the treatment confirm the induction? It does. Alkalies and neutral salts remove or cure rheumatism. Now it is not incumbent that this line of illustration should be entered upon, inasmuch as the major proposition stands proved, but it will bear a close scrutiny. The duration of the disease under the alkaline treatment is reduced one half, whilst under other treatment, the disease will average near three weeks' duration; under this ten, days or less.

Quibblers say the alkalies and neutral salts do not always cure rheumatism. Bœrhaave said there were no remedies in disease. The writer endorses the axiom, excepting specifics.

The claiming of a particular remedy fôr disease, or a treatment based upon one certain article for a certain train of morbid phenomena, outside of all the minute items, both patent and collateral, making and constituting the disease in part, is the claim and practice not of the empiric merely, but of the charlatan. This is not a remedy, then, in the sense which the quibbler or superficial thinker would give it. Having established the character of the materies morbi, no choice is left as to the department of chemistry or therapeutics from which to select your weapon. Acid requires its opposite; select that which is best.

Again, all other practices in the treament of this disease result in less satisfactory results. Take the list. Blood-letting, mercury, colchicum, opium, quinine, antimony, guiacum, propylamine, etc. These singly and combined have been used, and the result is before the world. Compare it with this. They fail in this, as we confess, gross test. Then having found the materies morbi to be an acid, an acid invariably present, it matters not whether lactic or lithic, the plainest dictate of science, would here meet the essential features, whilst all concomitant circumstances would receive due attention.

Exposure to cold and humidity have been given as cause and causes of rheumatism. Tested by the inductive process they fail; there is not an invariable presence to substantiate the claim. As remarked on a former occasion in the discussion of a similar subject, we want clear, succinct and simple statements; pointed, salient, and, if possible, axiomatic; strengthened by all the statistics that can be commanded; elucidated by all the arts and science of logic; bringing to its aid all kindred science, that the position taken may be placed among the truths of legitimate medicine. Does it, candid reader, take its place there?

A word or two more. Recognizing the great importance demanded by the tendency to carditis, the firm stand taken by the author as to the curability of rheumatic inflammation, with structural change of this vital organ, confers a great boon upon mankind. Physicians are too prone to speak despondingly of the result—a prognosis contradicted by the statistics of his experience. It is to be hoped that Fuller's work will stimulate the medical world to that zeal and energy which will rob the sufferer of much unnecessary pain and alarm, and cause them to be instrumental, through God's mercy, in snatching many from a premature grave. Talbot Owen.

MISCELLANEOUS.

"Non Omnes Eadem Mirantur Ament Que."

Case of Abnormal Gestation.* Read before the Richmond (Va.) Academy of Medicine. By M. H. Houston, M.D., Richmond, Va.—Owing to circumstances which need not now be mentioned, most of the memoranda connected with the following case were lost at the commencement of the late war, and its history is given principally from memory. Whilst many minute details, and possibly some important facts, may have thus been omitted, it is believed that it has lost nothing in the summary of its statements, as far as they are given.

Mrs. E. C. was married at the age of eighteen or nineteen to a young and highly intelligent practitioner of medicine. Although somewhat impaired by the training of opulent and indulgent parents, she had naturally a good constitution and was endowed with a strong will, and an indomitable and persevering energy of character.

Within twelve months after marriage she gave birth to a healthy daughter, the labor being normal, and the recovery rapid and complete. About two years afterward she gave birth to a second daughter, under circumstances equally favorable. Some months after her second confinement she began to complain, for the first time, of symptoms referable to derangement of the uterine functions. These continued for the next ten or twelve years, without being at any time so severe as, in her estimation, to demand medical attendance. Some time in the year 1852 she applied to me for advice in consequence of some sudden aggravation of her difficulties. The symptoms pointed clearly to uterine dislocation, but as it was supposed to be a simple prolapsus, from muscular relaxation, no vaginal examination was deemed necessary, and she was put upon a course of ferruginous tonics, with local astringents. Under this treatment she improved rapidly, and, after the lapse of a few months, menstruation was arrested under circumstances which induced the suspicion of pregnancy.

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*This very interesting paper is given without abbreviation.

Before the end of the next two months, uterine pains set in, which resulted in the discharge of what was supposed to be the product of conception. Not being in attendance at the time, I had no opportunity of examining the discharges, and, therefore, cannot pronounce with certainty. She was again put under constitutional and local treatment and at the end of a few months menstruation was again arrested, and the patient once more supposed herself to be pregnant. During the next six or seven months her general health improved steadily, and she experienced all the rational signs of pregnancy, except the motions of the fœtus, which, if felt at all, were acknowledged to have been doubtfully obscure. About the end of the seventh month of gestation, and some time during the year 1853, the patient, then living a few miles from the city, was taken with severe uterine pains, which, it was thought, must soon result in premature delivery. Under the use of an anodyne, however, the pains subsided, and the patient was directed to keep quiet for a few days. As long as she remained in bed, there was no return of pain, but when, in three or four days, she attempted to move about the house, the pains returned with increased violence, and, before I had seen her, there had been some serosanguinolent discharge from the uterus. It may be proper to remark that up to this time no doubt of the existence of pregnancy had ever entered either my own mind or the minds of herself or her attendants.

Upon making an examination per vaginam I found the os uteri occupying a position moderately high in the pelvis and to the left of the median line, in the direction of the left sacro-iliac synchondrosis. The whole right side of the pelvis was occupied by a large tumor, between which and the neck of the uterus was a sulcus, which might be due, as I thought, to the sharp bending of the uterus at the point where its body was joined to the neck, or it might be to a tumor which had pressed the uterus entirely out of its normal position.

A very careful examination led me to adopt the first suggestion. I thought I could trace distinctly the continuity of the neck with the walls of the tumor. In addition to this, the hand laid firmly over the body of the tumor never failed to detect contraction and relaxation during the continuance and subsidence of a pain, whilst synchronous with the contractions was a discharge of serosanguinolent fluid through the os uteri. With the view, if possible, of settling the question, I at once made a very careful stethoscopic examination of the whole enlargement, with the hope of being able to detect the pulsations of the fœtal heart. This I failed to do, and all hope from that source was reluctantly abandoned.

Although the fact had not attracted the attention of the patient herself, this examination proved that the main body of the tumor was to the right of the median line, and that it did not occupy the left upper portion of the abdomen at all.

Under the use of morphia the pains again readily subsided and the patient returned to a comparatively comfortable condition. The opinion I then expressed to her husband was that the fœtus was dead; that it was confined in the fundus of the uterus by the sharp curve at the junction of the fundus and cervix; that nature might ultimately overcome this obstacle, and that all we could do would be to wait patiently and meet the indications of treatment as they might present themselves.

From the time of the first discharge of fluids through the os uteri, until a short time before the period to which I am about to refer, there was a more or less gradual subsidence of the abdominal tumor, with a corresponding change in the position of the os; the latter gradually rising higher and receding farther in the direction of the left sacro-iliac sychondrosis.

To avoid repetition, I will now pass over several months of treatment, merely stating that the pains recurred with more or less frequency, and were accompanied with a discharge more or less copious, which, for some time, was highly offensive and characteristic of decomposing animal matter, but which gradually diminished and ultimately became almost entirely inodorous.

In October, 1854, and from sixteen to eighteen months after the supposed period of conception, Mrs. C. visited Philadelphia for the purpose of procuring the opinion and advice of Prof. Meigs. In my first letter to Dr. M. I refrained from any attempt to bias his opinion by giving my own, and merely stated that I thought the cause of difficulty was in malposition, with long continued adhesions of the uterus. In a written opinion, to the husband of the lady, the Doctor expressed the belief that pregnancy had not existed. The following letter, however, gives so full a description of the status of the case at that time, and the conclusions drawn from it, that it is given entire:

"PHILADELPHIA, October 19, 1854.

"MY DEAR SIR: Mrs. C. left the city to-day on her return journey, and I write now, both to thank you for your letter and say what impression was made upon my mind by a prolonged examination of the case.

There is certainly an obscurity, at present not to be solved, yet it seems, at least, apparent that the resisting mass to be felt through the abdominal integuments, in the vicinity of the superior strait of the pelvis, is not wholly composed of the womb.

"I examined the patient while lying on the back and found the os uteri to be situated so far backwards and so low down in the *excavation* that it is with a little difficulty to be reached by the index finger. When I *touched*, the os was at least three and one-half inches away from the crown of the arch of the pubis, which brings the posterior lip of the os within three-quarters of an inch of the fifth segment of the sacrum.

"The os is situated a little to the left of the median line of the sacrum, and if the cervix be traced upwards with the finger it is found to come sharply forward, inclining strongly to the right, as in this little figure. [Here a diagram is given.] I do not conceive that I have inclined it in the diagram more to the right than I found it on carefully touching for the diagnosis.

"The index finger can be very freely thrust upwards, in a direction to the left, toward the place of the left ovary, without giving pain or meeting much resistance; but it cannot ascend so readily on the right side within the cul-de-sac, or pouch of the vagina, to the right of the womb. Such pressure produces a very quick and distressing pain, and the resistance is not that of the mere areolar and adipose tissue enclosed betwixt the lamina of the right broad ligament, but is firm, and communicates, to me at least, a sense of some inflammatory turgor and elasticity there.

"This more solid resistance in the right posterior quarter-section of the pelvis and the absence of a similar state of things in the left quarter-section lead me to infer that a slow inflammation exists there that may end in a purulent deposit.

"Careful palpation of the left iliac fossa and the region about the left posterior third of the plane of the superior strait discloses there the existence of a voluminous and quite resisting body, which I cannot make out to be any thing else than a left ovarium much enlarged. Perhaps, however, it may not be an ovary, and I would not, by any means, insist that it is one, for the tympanitic state of the belly rendered it out of the question clearly to define the boundaries of the diseased mass. Perhaps you may be so fortunate as to obtain a more favorable opportunity to explore that region and so more accurately to determine the outline, and so, the nature of the mass. In tracing upwards the uterus from its os to the greatest height to which I could follow it with the point of the finger, I reached the conclusion that the womb does not now contain any foreign body, as a polypus or blighted ovum, and the form and size of the cervix appear to be of such a nature that I hardly doubt they are nearly normal. If not mistaken, I apprehend that the shape and size of the cervix is nearly like the annexed diagram, (here diagram is given), or rather smaller than the figure. I could barely distinguish the resisting summit of the womb, by pressing the fingers of the left hand down behind the body of the pubis, while a finger of the right hand touched the os within the vagina. Nevertheless, I must say, that the very firm resistance observed at the right side of the pelvis in exploring it with the index, may have deceived me, and so it may be that the right side of the womb is enlarged.

"I observe that Mrs. C. cannot rise from a chair without aid, nor walk without some support, because her knees give way. Do not these circumstances tend to strengthen the inference that an inflammation in the right posterior quarter-section of the pelvis does interfere with the pyriform and obturator internus muscles, and also contravene the power of the right anterior sacral nerves, and even so, impede the operations of the sciatic, popliteal and tibial nerves?

"But suppose it true that a dépôt is forming there, is there not ground to hope that the issue of the pus may be formed and allow the patient afterwards to recover her health?

Are the sudden, copious gushes of yellow matter she describes as having taken place, on various occasions, are they, I say, proof of a suppuration already existing? I know not, but I recommand an examination of the product, the next time it occurs, with the microscope, to determine the question as to pus or mucus.

"*As to treatment.*—It is clear that the lady should be well sustained by a good diet. She should take a fair portion of meat or broths daily. As for the rest I shall not trouble you with remarks. I believe it would be very prudent to give her half an ounce of oil of cod-liver three times every day. I suggest for her a course of the pilul Plummer, say 5 grains every night; followed by an aperient pill in the morning. Each of the aperient pills to be composed of 2 grs. of rhubarb, 1 gr. ext. hyosciam, and 1 gr. ext. gentian. Two or three such pills would probably serve the purpose in view—to procure at least one daily dejection.

"I have no present idea of the duration of the course of Plummer's pills. It certainly should not be allowed to develop any ptyalism.

"I would apply tinct. of iodine to the hypogastric region, and persevere in using it whenever the skin should not be so much irritated by it as to forbid.

Besides all the above, I beg to say that I would request her to take, every day, 5 drops of Lugol's iodine mixture, morning, noon, and night. A Sitz bath at 94° or 96°, would probably add to the comfort of the lady, and help, if possible, to cure her.

"These remarks, which seem to be all that at present suggest themselves to my mind, I lay before you, my dear sir, with a feeling of the greatest respect. If upon considering them, you should regard them as reasonable and hopeful as to the interesting patient, you will adopt some of them, otherwise your better judgment will lead you, I trust, to better methods, and so to a speedy relief from the bondage and pain of a disorder that excites sympathy in any one interested in Mrs. C.'s behalf.

"I now beg to assure you of the regard of your grateful and obedient servant, "CH. D. MEIGS."

In reply to this letter I gave, in full, my reasons for a difference of opinion. I dwelt particularly on the improvement in general health, which took place after the supposed conception, and urged that this was inconsistent with the idea of chronic organic disease of any kind. To give the whole letter would only be to repeat much of what has already been said. The following extract furnishes one or two points of interest, and contains a statement of what my opinion was at the time. The letter was dated the 25th of October, 1854, and after giving an account of my first visit tothe country, goes on to say: "A short time after this, a discharge took place from the uterus, having the character of, and concluded to be the liquor amnii. Whilst this discharge continued, the enlargement of the abdomen subsided. Next, after sharp uterine pain, something was discharged which *looked like an oyster.* Very much to my regret, it was not shown to me. The Doctor said it was certainly flesh, and I supposed it to have been a product of conception. An examination made a few days previous to this, disclosed the volume of the uterus much diminished and the os about half the distance from the situation which it occupied at my first examination, and the one which it now occupies. After the expulsion of the fleshy product, a dark, grumous discharge took place, variable in quantity and highly offensive in quality. An examination made at this juncture after the vagina had been well cleansed, and in which the index finger had been introduced partly within the os uteri, left on the finger, an unmistakable odor of decomposing animal matter, which could not be mistaken some hours after the examination. The offensive and dark colored discharge gradually ceased and gave place to the yellow discharge which still continues, though not constantly. Since the offensive discharge ceased, Mrs. C. has been gradually improving in health.

"So much for the history of the case. Now for the opinion which I had formed. I supposed that, for a long time previous to impregnation, the uterus had occupied a position in the pelvis similar, or nearly so, to the one which it now occupies, viz: the os far back in the direction of the left sacro-iliac synchondrosis and the fundus to the right, anteriorly somewhat, and in the direction of the tender prominence felt through the vaginal wall. I supposed that strong adhesions had taken place in this situation, with inflammation and thickening of the cellular tissue. The tumor on the left side I supposed to be the broad ligament put upon the stretch by the inclination of the uterus to the right side. The womb being confined in this situation, what would be the natural result of pregnancy? As the body of the uterus was developed, would it not have the effect of gradually throwing the os downward, forward, and nearer the median line? These adhesions, supposing them to exist, would admit of a certain extent of uterine development, whilst they would as certainly interfere to prevent a full development. As long as the womb furnished suitable accommodations for the growth within it, the lady enjoyed unusually good health. As soon as farther development was prevented, and the uterus was compelled to disburden itself of a load it was unable to carry, the other organs sympathized in the unnatural effort, and the whole constitution became the sufferer. Unless upon the supposition that one side of the body of the womb is fixed, how do you account for the change in the situation of the os, during different stages of development? I think the os is now at least two inches removed from the place it occupied at my first examination. Although I several times made the trial, with my index finger on the mouth of the womb and my left hand on the abdomen, yet I never could determine the exact situation of the fundus. The case has perplexed me very much, and although I cheerfully defer to your more enlarged experience and better judgment, yet I felt it due to myself to put you in possession of some of the facts which led me to a false conclusion, if such it should prove to be."

We now proceed with the history of the case. But, before doing so, it may be proper to remark that up to this time its whole medical treatment had consisted in the administration of morphia to relieve pain. The minimum quantity required was ¼ gr. every three hours, whilst occasionally it had to be increased to ½, and even to 1 gr. at the same intervals.

The treatment recommended by Dr. Meigs was commenced without delay, the medicines having all been prepared in Philadelphia under his supervision. Before doing so, however, I stated to her husband and to her brother-in-law, who was also an eminent retired practitioner, that I thought the Doctor had over-estimated the ability of our patient to sustain herself under active medication, and that I anticipated no good results from the change of treatment. The result more than realized my worst anticipations. Each day the patient rapidly grew worse, the pains became more and more severe, with increase of the uterine discharge; the abdomen became tympanitic; a fretful diarrhœa set in; loss of appetite, with nausea and vomiting supervened, and the whole condition of the patient became so alarming that I felt compelled to abandon the treatment and return to the use of the morphia, with hot fomentations and poultices to the abdomen. Under these appliances the unfavorable symptoms gradually subsided, and at the end of some weeks the condition of the patient was almost as favorable as it had been before the commencement of the alterative treatment. About this time Dr. C. wrote to Dr. Meigs, giving him an account of what had been done, and asking for further advice. In reply, the Doctor reiterated his former opinion and recommended a renewal of the treatment, with some modifications as to the use of the iodine. Finding that neither the patient nor her friends would be satisfied without another trial of the remedies suggested by Dr. Meigs, I agreed to renew the treatment, but with the distinct understanding that under no circumstance would I suspend it on my own responsibility, let the consequences be what they might, and that, if at any time they should desire its discontinuance, the suggestion must come from themselves.

The treatment was again commenced and persevered in for nearly two weeks, with results very similar to those witnessed on the former occasion. The condition of the patient was wretched in the extreme, and her friends scarcely entertained a hope of recovery. Under these circumstances Dr. C., the brother-in-law of the lady, called on me and stated that the condition of Mrs. C. seemed to be utterly hopeless, and that in a family consultation that morning he had been deputed to ask me to take entire control of the case, promising, in the event of my doing so, that my instructions should be fully carried out, without suggestion, modification, or interference from any quarter. Having great confidence in the recuperative powers of the patient and in the potency of morphia to relieve her of her sufferings, I cheerfully accepted the position and assumed the responsibility. It soon became evident that my confidence in neither had been misplaced. With hot fomentations constantly applied to the whole abdomen and ½ gr. of morphia every three hours, the violence of the pains was soon relieved, and when they ceased entirely the other organs were slowly restored to the performance of their normal functions. The certainty with which all the grave symptoms subsided upon the relief of the uterine pains was highly suggestive evidence of the *fons et origo* of all her calamities.

In the course of two weeks the tympanitic distention of the abdomen, which had been greater than the enlargement at the full term of gestation, was entirely gone; the pulse was reduced from 120 to 70 in the minute; the respiration, from being quick and anxious, had become natural; the skin, from being dry and heated, became soft and natural in temperature; the fretful diarrhœa was checked, and the operations assumed their natural appearance; the uterine discharge, which had been much increased, diminished in quantity and finally ceased, except at irregular intervals; the nausea and vomiting subsided, and the stomach, with a healthy digestion restored, soon asserted her empire over the whole economy. This occurred during the winter of 1855–6.

Through the spring and summer her general health contined to improve steadily, and in August she was so far restored as to be able to pay a visit to the Berkley Springs.

From a stray leaf, containing in pencil mark a portion of the original draft of a letter written to Dr. Meigs at this time, I make the following extracts: "When I last made a vaginal examination, the os uteri occupied the same situation in the left side of the pelvis which it did when you saw her. With the index finger I could trace the wall of the uterus until it terminated in the tumor on the right side. There was no tumor to be felt on the left side, either internally or externally. Externally, on the right side, the tumor occupied a position between the symphisis pubis and the anterior superior spinous process of the ilium. It was very slightly movable and diminished in size during pains referable to uterine contractions. There is still some tenderness on pressure and occasionally there are some paroxysms of pain. Some months ago, the catamenia made their appearance, and for the last four months she has menstruated regularly, though, perhaps, not in the quantity which she formerly did. She walks about and attends to most of her domestic duties, though she cannot walk far or stand long without suffering inconvenience. Her general health is quite as good as it has been for years, and, to look at her, no one would suppose that she claimed to be an invalid. She has recovered her flesh, her complexion is good, and she generally enjoys a fine flow of spirits."

The letter closed with the following queries, to which no answer was returned:

"What is your opinion of the present condition of the pelvic organs? Do you think it possible that there are still some fœtal remains in the cavity of the uterus, and that nature is gradually accommodating herself to the abnormal state of things?

"Is regularity of the catamenia consistent with such ovarian disease as you supposed at one time to exist?

"Would the treatment, which has proved so beneficial, have had any effect in discussing ovarian tumors?

"May not the ovarian enlargement in the left side have been due to simple engorgement from sympathy with long continued uterine irritation?

"What will probably be the issue of the case?"

This brings the history of the case down to the summer of 1856. The condition of the patient continued to be very much the same until some time in the following autumn, when she had an attack of typhoid fever. Having been so fortunate as to recover one of my own letters, which gives a concise account of what took place from this period until the day of its date, I give it entire:

"WHEELING, July 17, 1857.

"*To Ch. D. Meigs, M. D., Philadelphia:*

"DEAR SIR: Some time last summer, I gave you a continuation of the history of Mrs. C.'s case until the date of my letter. Although you have not requested me to do so, yet I take it for granted that my giving you the particulars of this very extraordinary case, as it progresses, will not prove unacceptable.

"When I last wrote, Mrs. C. was in the enjoyment of a very tolerable degree of health. She continued to improve, and, during the latter part of the summer and most of the fall, she rode out, walked, visited her friends and attended to most of her domestic duties. In the latter part of the fall she was attacked with typhoid fever, which was somewhat prevalent at the time. The symptoms and progress of the disease were scarcely different from those which present themselves in ordinary cases.

She did not, however, recover as promptly as I hoped she would. As the fever subsided and her stomach recovered its tone, evidences of more than usual irritation in the tumor and about the head of the colon manifested themselves. The soreness in the tumor itself and the irritation of the bowels were such as to demand the more free and constant use of morphine and the almost unceasing application of warm fomentations and poultices. Some time in March there was a colored discharge from the uterus, supposed to be menstrual, and at the same time a discharge of mucus and blood from the bowels, supposed to be from hæmorrhoids. Upon examination I found that the tumor had diminished in size and that it was retreating still farther within the cavity of the pelvis, whilst there was a new tumor at the caput cœcum coli. This last I supposed to be from fœcal accumulation and directed my remedies accordingly. It is only necessary to say, that the action of laxatives proving unsuccessful and their use producing much pain, with tympanitic distension, I resorted to $\frac{1}{2}$ gr. doses of morphia every three hours with the effect of unloading the colon of its contents within the next twenty-four hours. The operations were large and were reported to me as consisting of hardened scybalæ. Similar accumulations took place from time to time, and were removed by similar means. I sought frequently an opportunity to examine the discharges, but owing to circumstances it was never obtained. During the latter part of June the accumulations were principally about the sigmoid flexure, and I did not hesitate to express the opinion that they were due to some mechanical obstruction. About the 1st of July Dr. C. came to me and said something very singular had happened with his wife; that whilst having an operation something had engaged itself in the sphincter, which was hard and felt like a ring; that she had broken it with her finger, and had removed a portion, whilst a portion was left behind. I immediately asked to see the portion removed. He said he believed it had been thrown away. I then told him I suspected it to be a fœtal atlas, and requested him to go home immediately and make diligent search for the fragment removed, and, if any remained, to send for me at once. In a few minutes I was sent for and was told that something still remained in the rectum, while they handed to me three small fragments of bone which Mrs. C. had succeeded in removing herself. Upon introducing my finger into the rectum I recognized the presence of what I supposed to be a parietal bone, with edges ragged and almost as sharp as a knife. Every motion of it gave her most excruciating pain. The difficulty was, how to get it through the sphincter, without causing too much pain. With such appliances as were at hand, an attempt was made to dilate the sphincter, but finding that the bone had receded some distance, and that it caused no pain unless when touched I determined to let it rest until I could be better prepared. At the end of thirty-six hours I made a successful effort to remove the bone. It was impossible to operate through any of the specula in common use. To crush the bone suggested itself of course, but it was no easy matter to use an instrument for that purpose, even if one could have been found capable of crushing so flat and hard a substance. A very simple expedient suggested itself, which answered the purpose admirably. With a few minutes' assistance from a silversmith, two common tablespoons furnished me with the exact instruments required. The handle was bent at right angles just where the narrow expands into the broad portion, whilst the broad portion was bent longitudinally into the segment of a tube large enough to embrace the end of the finger. The patient being properly situated, these spoons were introduced into the rectum and entrusted to an assistant on each side, with instructions to draw them asunder gradually, but steadily. As soon as possible I introduced my finger between them and found the bone, with its sharp edges, engaged in the folds of the mucous membrane. It was detached, and by a little careful manipulation I succeeded in getting its sharp edges fairly within the hollow of the handles on each side, when, with a pair of long, slender forceps, I slowly drew it along its road-bed and made it to pass the sphincter without pain to the patient. It proved to be, not a parietal, but the occipital bone. At the same time I removed several smaller fragments, either of this or of other bones of the head.

"Not to weary you with details, I may mention that since that time, the patient has passed, without pain, or extracted for herself, one scapula, one ilium, two tibia, one fibula, seven ribs, besides other fragments not so well defined. How many bones may have passed before the first was detected, it is impossible to conjecture. It is evident, however, that others are still in the neighborhood of their entrance into the bowel, whilst some seem to be making their way along the arch and lower portion of the colon.

"Just at this time she is suffering more than usual pain in the region of the sigmoid flexure, and I fear that more bones of the head, with sharp, ragged edges, will yet have to be dislodged. During all this time Mrs. C. has been sustained with a remarkable degree of fortitude and courage. She enjoys good spirits, and, all things considered, her appetite and digestion are good.

"The fact of the existence of pregnancy is now placed beyond controversy, and the question remains, how did these bones find their way into the colon, and where have they remained imbedded during the last two years? On a former occasion I expressed the belief that the fœtus occupied the fundus of the uterus, and nothing has since occurred to cause me to change this opinion. The reasons for that opinion are contained in my former letter, and it may not be necessary to repeat them. With the developments now before us, what is you opinion? Do the records of medicine contain any parallel case? It will give me pleasure to furnish you with any additional facts in my possession which you may designate as necessary to a full understanding and appreciation of the case, and, if you desire it, I will send you all the bones of which I can obtain possession. I may remark that they are thoroughly cleansed of all the soft parts, and probably have been so for a length of time.

"May I ask of you the favor to write to me as soon as convenient and to furnish me with some suggestions as to the proper future management of the case. May I alse request you to preserve my letters, as they contain the history of the case as it progressed.

"Very respectfully, your obedient servant,

"M. H. Houston."

In reply to this I received a long letter, the loss of which I very much regret, as it contained a lengthy and very learned dissertation on the subject of extra-uterine pregnancy generally. I think I am not mistaken in saying that he ignored the whole doctrine, and asserted that in all cases where a fœtus, beyond a certain stage of development, had been found in the abdominal cavity, there existed material evidences of its having passed from the cavity of the uterus. He also stated that he had examined all the records, within his reach, without finding any case similar to the one under my care.

The subsequent history of the case may be given in very few words. In August 1857 the patient had an attack of cholera morbus, whilst on a visit in the country, which very nearly proved fatal. She was attacked in the evening, and, notwithstanding the use of gr. doses of morphia, with teaspoonful doses of laudanum in brandy, with the other usual appliances, it was day light the next morning before she was relieved.

On the following morning, when I visited her, she met me with a smile and presented me with three or four bones, as trophies of her morning's work. From this, forward, she continued to improve in general health, occasionally recovering a bone from the dejections, until 1858, when she removed to Missouri. During her residence there I am not sure whether she discharged any bones or not. In 1860 she returned to Virginia in the enjoyment of good health, and informed me that she experienced no inconvenience from her former sufferings, except under particular circumstances, which it may not be uninteresting to mention. On several occasions, after eating freely of raspberries, she was annoyed with the old pain and enlargement in the region of the caput cœcum coli. She relieved herself by manipulations, with which she had become very familiar, and found in the digestions which followed, one or two teaspoonsful of raspberry seed. This may be accounted for on the supposition, that a cul-de-sac remained, after the closure of the opening, through which the bones had passed into the bowel. At all events, she was deterred from ever indulging in fruits containing small seeds.

After the commencement of the war in 1861, Mrs. C., with some other refugees, removed to the central part of this State. Whilst sojourning at the Natural Bridge, she was attacked with a cough which resulted in fully developed tubercular consumption. She died in 1864 under circumstances which precluded the possibility of a post mortem examination.

It may be worthy of remark that, with the exception of the two periods, during which she was subjected to alterative treatment, Mrs. C. was constantly under the influence of morphine for more than three years. The smallest quantity which afforded her comparative comfort was ¼ of a gr. every three hours, or 2 grs. in the twenty-four hours. It frequently happened, however, that the dose was increased to ½ gr., and sometimes to 1 gr. every two or three hours. It seemed to be the only remedy capable of meeting every indication which presented itself. When diarrhœa supervened, it allayed irritation and checked the discharges, and when the bowels became constipated and when impacted with bones and indurated fæces, it could only be relieved by increased doses of morphine.

Notwithstanding this long continued use of the anodyne, as soon as the cause of the pains, which rendered its administration necessary was removed, it was discontinued, without an effort and without the occurrence of a single unpleasant consequence.

Minutes of the Meeting of the Richmond Academy of Medicine, April, 1868.—The Academy was called to order at 8 o'clock p. m., President Gaillard in the chair. Present—Drs. Houston, Haxall, Thomas, Fairfax, Fuqua, Watkins, Tabb, James, Dickinson, Cunningham, Skelton, Snead, Bolton and Ross.

The minutes of the last regular semi-monthly meeting, held the 19th of March, 1868, were read and approved.

A report of cases being in order, Dr. Houston read an exceedingly interesting report of an obscure case of pregnancy, which terminated in the gradual expulsion and removal of the bones of the fœtus, per rectum.

Dr. Fuqua then read an article on the action of digitalis, strongly disapproving of its administration in practice, and giving his own experience as unfavorable.

Dr. Houston, in his report, having made reference to chloroform, Dr. Haxall was reminded by it of the case of a young lady, under charge of Dr. Garnett, suffering greatly from dyspepsia, nausea, constipation, &c., whom he had seen and to whom relief could only be afforded by the administration of purgatives, enema, &c. Some difficulty was experienced in the introduction of the syringe, which, upon examination, proved to be due to an immense impaction of fæces, forming a large tumor. The patient was put under chloroform, and the entire hand introduced for its removal.

Dr. Fairfax had seen a middle aged lady, whose bowels were regularly moved, but in whom a vaginal examination detected a tumor, as large as the head of a child, entirely filling up the pelvic cavity. This also proved to be fæces, and was readily removed with a spoon. He supposes a channel had formed alongside the mass for the daily evacuation.

Dr. Gaillard, having requested Dr. Fairfax to take the chair, said that, with due deference to the long experience and better judgment of Dr. Houston, he did not think the case, an account of which had just been read, was one of intra-uterine pregnancy. He did not think the facts, as narrated, proved it. No fœtal bones passed away with the discharges, and there was no period characterized by such prostration, as always marks the rupture of the uterus, when the fœtus passes into the abdominal cavity. He thought it a case of tubo-uterine pregnancy. Ventral pregnancy was now rejected, and ovarian pregnancy, though possible, was very rare. There were three kinds of extra-uterine pregnancy, tubo-ovarian, tubal and tubo-uterine, and he thought this was a case of tubo-uterine pregnancy. The uterine enlargement was not peculiar. Extra-uterine pregnancy produced the same uterine changes, as did intra-uterine pregnancy. The same deciduous membrane was formed in extra, as in intra-uterine pregnancy, and the placenta was at times attached to the fundus uteri, in extra uterine pregnancy. He thought that grave symptoms would have attended the laceration of the womb, and that some of the bones would have escaped per vaginam, as well as per rectum.

Dr. Haxall said that the lateral flexion of the womb might have prevented the passage of any bony fragments by the vagina.

Dr. Snead said that the flexion was not sufficient to prevent the escape of fluids, but might of bones. He agreed with Dr. Gaillard; the case was one of interstitial or tubo-uterine pregnancy, a part of the placenta being attached to the fundus uteri.

Dr. Houston said that, if the placenta and part of the fœtus were in the womb, 'twas still intra-uterine pregnancy. If Dr. G. was right, the fallopian tube must have been ruptured. He believed it to be intra uterine, because, from the time of the supposed conception, until the beginning of the discharge, the patient's health was better than it had been for years, because the womb dilated regularly and because *all* the soft parts decomposed and passed away through the os uteri. The discharge was at first dark, grumous, as in decomposing placenta, and then of a lighter color, as in decomposing animal matter. The womb had been displaced some time before conception, and he thought that adhesions of the fundus had taken place. If that were true, as soon as the development of pregnancy began, the cervix was passed to the left, the fundus falling to the right and producing such contraction, as to prevent the passage of the bones by the natural outlet. He thought that the bones, after remaining in the cavity for three years, had ulcerated through the womb and into the colon, and thence passed out. No better explanation of the symptoms and termination of the case was afforded on the diagnosis of tubo than of intra uterine pregnancy.

Dr. Watkins said that, inasmuch as this patient had for three years been constantly under the control of morphia, and muscular action thereby prevented, he thought it perfectly natural for the bones to ulcerate through the womb so simply and slowly, as not to produce symptoms more marked than those described by Dr. Houston.

Dr. Gaillard regarded the case as very obscure. He did not think that the natural elevation and enlargement ot the womb proved that the case was not extra uterine, nor did the pains as of labor at full term, because the same pains attended extra uterine pregnancy; that the flexion was due to such pregnancy, physical laws would demonstrate. He conceived the fœtal development to have been so gradual, as to have produced a slow absorption of the tubo-uterine tissues, without those violent symptoms attendant upon rupture of the womb. If the fœtus had decomposed in the cavity of the womb, some of the bones ought to have been discharged per vaginam, as well as per rectum. The attachment of the placenta to the fundus would account for some of this discharge.

Dr. Houston asked why the bones did not follow the soft parts through the os uteri.

Dr. Gaillard replied that the fluids could pass downwards by gravitation, while the bones remained at the point of attachment, the uterine lesion being slight.

Dr. Houston thought the ulceration took place at the point of adhesion of the womb to the abdominal contents, including the colon, and that no part of the fœtus ever passed into the abdominal cavity proper.

Dr. Haxall could not see how the placenta ever formed in the fundus uteri, if the ovum never reached there.

Dr. Gaillard said such cases were recorded.

Dr. Snead still thought it tubo-uterine, or interstitial pregnancy, and that this diagnosis was quite sufficient to account for the lateral obliquity of the womb.

Dr. Houston thought that, if the adhesions had not taken place, there would not have been the binding of the womb at the union of the body and cervix, and there would have been a correspondent development of the left side of the abdomen.

The evening being consumed, the question for discussion, "alcoholic stimulants," was postponed until the next meeting.

On motion of Dr. Haxall, the Academy adjourned.

GEO. ROSS, M. D., Secretary.

SIR JAMES SIMPSON'S EULOGY UPON SIR DAVID BREWSTER.—At a recent meeting of the Royal Society of Edinburgh, held in reference to the death of Sir David Brewster, Sir James Simpson, after presenting a series of resolutions, made the following remarks, which are copied from the Canada Medical Journal of April:

Perhaps the Society will kindly bear with me, while I venture to add, to these resolutions, a very few remarks. With you, Mr. President, I hope our colleague Professor Tait, will, at some early meeting, give us a full *résumé* of all the wondrous discoveries and inventions in science, especially in optical science, which we owe to the genius and researches of Sir David Brewster. But permit me to make one or two observations on other matters, not scientific. As early as the age of twelve, Sir David became a student at the University, and he was still a student in every true sense of the term for the subsequent seventy-five years of his life. While yet only ten years old, he constructed a telescope (with his friend Mr. Veitch) significant of the chief future bent of his work and genius. Few men, we all know, have ever been more successful in unfolding and revealing some of the most hidden and obscure laws of science. His self-imposed task only ended with his life. And on this subject, it seems to me that I carry almost a mission from him to us—from the dead to the living; for when I last visited him at Allery, when he was within a few hours of his death, when he was already pulseless, his mind was perfectly entire, and perfectly composed; and on asking him, among other matters, if he wished any particular scientific friend to take charge of his remaining scientific papers and notes, he answered me, "No, I have done what every scientific man should do—viz, published almost all my observations of any value, just as they occurred." He then explained that he had left one paper on "Films" for the Society, and then went on to express an earnest regret that he had not had time to write to the Society another description of the optical phenomena which he had latterly observed in his own field of vision, where there was a partial degree of increasing amaurosis, which, he thought, might be yet found a common form of failure in the eyes of the aged. He described the appearance of this partial amaurosis minutely and energetically, telling me, for your hearing, that "the print of the Times newspaper had begun, for a year or two past, to look at one part in the field of vision, as if the white interstices between the letters were lightly peppered over, with minute dark powder;" and this amaurotic point was, he observed, latterly extending, like the faint extending circle around a recent ink-blot on blotting-paper. The clearness, vigor, and energy with which he detailed all this and more were amazing in one already so very weak, and so very near the last ebb of the tide of life. Then let me say further in relation to him, as a philosopher, that his death has broken several curious links with the past. For example, as I was told when down in that part of the country, he was the last of the stars that forty years ago dwelt on the banks of the Tweed, and formed the constellation of friends that clustered there round the Great Wizard of the North at Abbotsford. In the first years of this century—1802–1803—he was much with Cavendish, connecting us thus with the grand band of philosophers who lived in the metropolis of England. Married to a daughter of Macpherson, he connected us with the time, more than a hundred years ago, when Johnson, Blair, Home, and others, disputed so acrimoniously as to whether Ossian's poems were true or not. I would make only one remark more. Professor Fraser told me this day, what I have heard, before with regard to his great precision, energy, and determination of thought—that during the seven years that he (Professor Fraser) was editor of the North British Review, Sir David Brewster contributed an article to every number; and that he did far more—that he stated the day, when the first slip of paper would come, and the day when it would be finished. His manuscripts came as they were written—day after day, and sheet after sheet—and without the necessity of the revisal of those preceding. He thus worked with the precision and regularity of a mechanical, rather than a mental machine. Sir David Brewster must have been originally endowed with a robust and iron constitution. Few men ever reached the age of eighty-seven with an intellect so unimpaired and an ear so acute. In later years, however, he had repeated attacks of serious illness. But since he attended the meeting of the British Association at Dundee in Autumn last, where he was carried out from one of its crowded meetings, in a state of syncope, his health has rapidly declined. He died, ultimately, of an attack of pneumonia and bronchitis. A rigor, ushering in the fatal illness, occurred eight days previous to death. From the date of its occurrence, he felt and stated that the grasp of the hand of death was now fixed upon him. Yet, though feeble and weak, he insisted upon being allowed to rise and work for a few hours daily. Three days before he died, he insisted on dressing and going into his study, where he dictated several farewell letters, and amongst others, one to our secretary, Dr. Balfour. "Permit me," he plead with those around him, "permit me to rise once more, for I have work to do." "I know," he added, "it is the last time I shall ever be in my study." Towards the end of that day's work, his friend and pastor, Mr. Cousin, visited him. "My race," said he to Mr. Cousin, "is now quite run. I am no longer of use either to myself or to others, and I have no wish to linger longer here." "Yet," he added, after awhile, "it is not without a wrench, that one parts from all those he loves most dearly." To Mr. Phin, and other clerical friends, he freely expressed in these, his last days, the unbounded and undoubting faith of a very humble and very happy Christian. No shadow of dubiety ever once seemed to cloud his mind. Like my former dear friend and old school companion, Professor John Reid, he seemed to be impressed with the idea that one of the great joys and glories of Heaven would consist in the revelation of all the marvels and mysteries of creation and science by Him, by whom "all things were made," and who, as Professor George Wilson held it, was not only the Head of the Church, but the Head and origin and source of all science. "I have," he remarked to me, "been infinitely happy here; but I soon shall be infinitely happier with my Savior and Creator." As death drew more and more nigh, the one idea of his Savior, and of his being speedily and eternally with Him, grew stronger and more absorbing. A near connection, but not a relative, who, in former years often lived in his house, and latterly formed one of the loving watchers by his death-bed, mentions this characteristic and striking anecdote: "When we were living in his house at St. Andrew's, twelve years ago, he was much occupied with the microscope, and, as was his custom always, he used to sit up studying it, after the rest of the household had gone to bed. I often crept back into the room, on the pretence of having letters to write, or something to finish, but just to watch him. After a little, he would forget that I was there, and I have often seen him suddenly throw himself back in his chair, lift up his hands, and exclaim, 'Good God! Good God! how marvellous are thy works.'" On Sunday morning I said to him that it had been given to him to show forth much of God's great and marvellous works, and he answered, "Yes, and I have found them to be great and marvellous, and I have felt them to be His." As a physician, I have often watched by the dying; but I have never seen a death-bed scene more full of pure love and faith than our late President's was. His death-bed was indeed a sermon of unapproachable elequence and pathos. For there lay this grand and gifted old philosopher, this hoary, loving votary, and arch-priest of science, passing fearlessly through the valley of death, sustained and gladdened with the all-simple and all-sufficient faith of a very child, and looking forward with unclouded intellect and bright and happy prospects to the mighty change, that was about to carry him from time to eternity. I hope the Society will kindly excuse me if I have dwelt too long on the task which the Council has imposed upon me. May I be permitted to add one single remark more. Sir David Brewster appears to have left, for us all, a strong and touching and marvellous lesson alike in his life and in his death. In his life he has shown us what a gifted and gigantic intellect can effect, when conjoined with industry and energy, in the way of unvailing and unfolding the secret laws and phenomena of nature. In his death he has shown us, that one possessing an intellect so gifted and so gigantic could possess and lean upon the faith of a pure and simple-hearted Christian. That faith made to him, the dreaded darkness of the valley of death, a serene scene of beauty and brightness. May God grant that it do so to every one of us. His spirit even now seems to me to be beckoning on the votaries of literature and science, here and elsewhere, along that path which he has so gloriously trod, upwards and heavenwards and Christwards.—[*West. Jour. Med.*

COLORLESS TINCTURE OF IODINE. By HUBERT PRIMM, Ph. D., Professor of Practical Pharmacy in the St. Louis College of Pharmacy.—Having had frequent calls for a colorless tincture of iodine, and the formula in use presenting decided therapeutic objections, I have devised the one which will be found at the close of this article. There are two formulæ in general use, the first being made as follows:

Iodine	℥j
Alcohol	f℥xij
Water of ammonia	f℥iv

Dissolve the iodine in the alcohol, and add the water of ammonia. Let stand until bleached, and filter. The result of this process is the formation of an alcoholic solution of iodide of ammonia, with perhaps some iodate, and contaminated with the presence of caustic ammonia, which prevents the colorization of the product.

It is obvious that the use of this preparation can but be limited, for if we recollect that properly prepared aq. ammonia contains ten per cent. of ammonia, it requires but little forethought to see that there would arise many cases in which, though the use of tr. of iodine would be indicated, yet owing to the caustic nature of this preparation, its employment would be dangerous. The second formula is that of Dr. Percy Boulton, and is made as follows:

℞.—Compound tincture of iodine	46.5 grs.
Liquid carbolic acid	6 drops.
Glycerine	ʒvij+45.5 grs.
Distilled water	℥iv+ʒv+37.5 grs.

M.

This formula presents some very valuable therapeutic advantages, combining, as it does, the deobstruent qualities of the iodine with the peculiar antiseptic properties of the carbolic acid, and, no doubt, in practice will fill up a vacuum that has long existed, as there are many instances in which the combination of iodine and carbolic acid would be peculiarly advantageous.

The objection that can be urged against its use lies in the fact that, to many, especially to women, the intolerable odor of the discoloring agent is as disgusting, as is the disfiguring effects of the officinal tr. of iodine, besides which I have not, as yet, met with any sample of carbolate of iodine which is perfectly colorless—they were all more or less of a light yellow hue. It will also be perceived that there is but a small per centage of iodine in each fluid drachm of the preparation, which rather unfits it for external use.

In the formula herewith submitted, the writer, without arrogating to himself much claim to originality, thinks that by the use of his process many of the objections that are urged against the use of the formulæ above presented will be obviated. It is as follows:

℞.—Tincture of iodine, U. S. P. : : :	fʒvj
Distilled water : : : : :	fʒii
Hyposulphite of soda : : :	108 grs.

Triturate the soda with the water and add the tr. of iodine gradually, with constant stirring. When the process is completed, filter.

The resulting tincture is a clear and limpid solution, possessing no causticity beyond that naturally pertaining to all alcoholic solutions of iodine; and, as far as I have been able to learn from those who have used it, has all the resolvent, absorbent and deobstruent properties that belong to the officinal tincture of iodine.

In conclusion, the writer of this is well aware that the preparation of a colorless tinct. of iodine, which would represent iodine *per se*, without the formation of a new chemical compound is, as yet, a chemical impossibility; but he does claim that, as long as the medical profession have to have recourse to combinations of iodine with decoloring agents, the one which will present it the freest from causticity and odor is the best.—*St. Louis Medical Recorder.*

MEDICAL NEWS.

"Nulla Dies, Sine Linea."

Medical College of Virginia.—Dr. Otis F. Manson has been elected to the chair of General Pathology and Pathological Anatomy, made vacant by the resignation of Dr. E. S. Gaillard.

Dr. Manson is a practitioner of matured experience; an accomplished physician; a graceful and facile writer; a student and scholar, and one who will, it is believed, efficiently and faithfully discharge the duties of his position.

The Board of Visitors have created another new chair, (making nine), that of the Diseases of Women and Children, and have elected to it Dr. Walter S. Coles, of Parkersburg, W. Va. Dr. Coles is not personally as well known to the writer, but, from general testimony, there can be but one opinion, as to his entire fitness to creditably perform the arduous labors, thus imposed upon him.

It should have been mentioned, in the last issue of this Journal, that Prof. Beverly R. Wellford had (from the results of sickness) resigned the chair of Materia Medica and Therapeutics, which he has, for many years, filled with honor, credit and efficiency. He has been happily and most deservedly made Emeritus Professor. His son, Dr. J. Spottswood Welford, has been elected to succeed him, and having had two years' experience, as a teacher of those branches, he has great advantages in his future and very honorable career.

The American Medical Association has recently (May 5th) held its nineteenth annual meeting, Washington being the city selected for this purpose.

The meeting was, in all respects, happy and harmonious. Physicians, from all sections of the United States, were present, and, from all accounts, fraternization was conspicuous and complete. As has been urged before in this Journal, it was eminently proper that Southern physicians should attend this meeting, and no one present can regret the course pursued. Prof. Gross, the President, delivered an able address and resigned the chair to Dr. W. O. Baldwin, of Alabama, who was previously elected President for the next year. The Vice Presidents are as follows: Geo. Mendenhall, of Ohio, 1st; N. Young, District of Columbia, 2d; P. Monroe, of Maine, 3d; S. M. Bemiss, of Louisiana, 4th.

The meeting is said to have been a great improvement on those of the past few years. There was less junketing and more work, and the indications are, that future meetings will contribute, once more, something of real value to the profession. Further particulars will be given in the July issue.

Prof. J. J. Chisolm, of Charleston, S. C., intends making Baltimore his *summer* home, in future, returning each winter to discharge his professional and professorial duties in Charleston, S. C. Prof. Chisolm expects, during each summer, to devote his attention to Ophthalmic Surgery. He has had every experience in this specialty, and will, doubtless, soon obtain, in his present home, a deserved success.

Louisville Summer School of Medicine.—Preston B. Scott, M. D., Obstetrics and Diseases of Women; L. P. Yandell, Jr., M. D., Materia Medica and Diseases of Children; Coleman Rogers, M. D., Principles and Practice of Medicine; R. O. Cowling, M. D., Principles and Practice of Surgery; E. R. Palmer, M. D., Anatomy and Physiology; L. D. Kastenbine, M. D., Chemistry and Toxicology. The regular course of lectures began March 10th and will close September 30th, 1868. The daily examinations and demonstrations are continuous throughout the year. The course will embrace one daily clinic, two daily lectures and one daily examination.

Louisville Preparatory School of Medicine and Surgery—Conducted by B. B. Nesbit, M. D., H. Clay Lloyd, M.D., Geo. J. Cook, M.D. The first session of this institution commenced on Monday, April 6, 1868, and will continue until Saturday, September 5, 1868. The second session will commence on Monday, October 5, and continue until the close of the session of the Kentucky School of Medicine.

These excellent schools are now in daily operation and students will find it greatly to their advantage to attend either of them.

Death of Prof. Pirogoff.—The death of this world-renowned surgeon took place a few days ago under extraordinary circumstances. Being at Odessa, Pirogoff was called in consultation to a patient near that city, and on his return was attacked by a gang of highwaymen. He killed two of them, while the others escaped, and he pursued his journey, feeling that he had narrowly escaped the danger of being murdered. But when he reached his house, Pirogoff was seized with symptoms of cerebral congestion, and he soon after expired from the effects of the murderous attack.

Cinnabar, of a beautiful vermilion color, is found in an unusual form in Idaho, being abundantly spread throughout a gangue so massive, compact and homogeneous, that specimens may be cut and polished like marble.

EDITORIAL.

"Nullius Addictus Jurare in Verba Magistri."—Hor.

In bringing this Journal into a new field; in sending it to those who have heretofore not received it, and in associating in the responsibility of its management, those gentlemen, whose names appear on the title page, a few remarks will, naturally, be regarded as necessary and appropriate.

Some physicians have anticipated that this work would, in a direct or indirect way, be used for promoting the interests of the Medical School, with which the editor is now identified. It is proper to say, that such anticipations will never be realized. The object of a medical work would, by such a course, be irredeemably prostituted, and the labors of all connected with it would, justly, become contemptible. Others may entertain a different opinion; such is the conviction of the writer, and upon such a statement is he willing to have the future course of the work submitted for judgment.

Since the date of its original publication, the Journal has been devoted, exclusively, to the progress and development of Medical Science. Nothing of a personal, political, or controversial character has been published in its pages, and this rule will be preserved inviolate. It has, under varying circumstances, been independent of all individual or corporate interests, and it will always so remain, the editor seeking, only, to make it an exponent of the scientific views of the whole profession. Physicians everywhere, therefore, are respectfully invited to contribute to its pages.

A journal, whether medical or secular, must, to a great extent, be a reflex of the various subjects which engross the professional mind. These subjects are infinitely various, and, apart from a close attention to the harrassing duties and mechanical details, incident to journalism, the difference between success and failure in a work is largely due to the judgment displayed, in the selection of subjects engaging the attention of physicians. In the present days of mere book-making and of pretension; of dogmatic assertion and servile obedience; of complex philology and pedantic phraseology; of fondness for all transcendentalisms in theory, practice and expression; in the growing disposition to imitate German vagueness, ambiguity and mysticism; to parade skepticism, either as a shield for ignorance, or as a ludicrous effort to be regarded as profound; in the increasing tendency to cultivate metaphysics in medicine and to conceal impotent investigation by adopting a meaningless technology; to prefer disputation to demonstration; to mistake the chimeras of the microscopist for revelation and discovery; in these days when phantasy has almost usurped the sovereignty of philosophy and the medical mind is so frequently willing to accept complex fictions, as the logic of fact, the difficulties of journalism are infinitely increased. The physician of the library wishes matter that "smells of the lamp," logical efforts to support recondite theories, abstruse dissertations and exhaustive essays, while the active practitioner desires convenient and useful formulæ, practical suggestions, useful hints, and a concise expression of facts.

Can the editor of a journal reconcile these conflicting elements? This may be done, by a happy judgment in regard to eclectic matter, but it can be chiefly secured, by an appeal to the practical and thinking men of the profession. In the cities, there are those who see much, read much, analyze quickly, and assimilate well. In the country there are many who, from the nature of their duties, are much from the library, but who read with study, who think carefully, who observe closely, who are independent in thought and wonderful in judgment. It is from both of these classes that a journal draws its vitality and the profession derives so much of its instruction. Contributions from such men are what is chiefly needed to give a journal practical value, to give it strength and its readers wisdom. It is to this large class that the editor appeals, to give to give this Journal that practical character which will make its dissemination a source of instruction and its success a cause of genuine satisfaction.

REMOVAL TO LOUISVILLE.—As soon as the last portion of the May Journal was ready for the press, the editor of this Journal received a notification of his election to the chair of General Pathology and Pathological Anatomy in the Kentucky School of Medicine. Resigning a similar position in the Medical College of Virginia, arrangements were immediately made for transferring the Journal to Louisville. All business details, for the future publication of the work in this city, have been perfected with all possible celerity, and, it is believed, in a manner that will improve the general appearance of the Journal and make it more acceptable to its supporters. After much labor, this number is issued promptly on the first of June, and it is expected that all future issues will be made with punctuality.

To the Exchanges of this Journal.—There has never been a more pleasing duty, devolving upon the editor of this Journal, than that of placing upon record his warmest acknowledgments and thanks, for the kind and very flattering remarks which have been made, in the medical and secular publications of the country, on the occasion of the change in the title of this work, and of its removal to Kentucky. To all contemporaries who have sent their welcome words of commendation and encouragement, it can only be said, that their language is particularly welcome and grateful. They best know what are the constant trials and the barren rewards of journalism, and they best understand why words of appreciation and kindness, from the journalist, are esteemed and welcomely remembered.

A few of the exchanges have omitted to notice the removal of this Journal. Among the thousands, who take pleasure and derive profit in justly supporting these publications, there are a few who, though not subscribers to this work, are yet its supporters. It is desired that these should be informed of the change in the title of the Journal and in the locality of its publication. Will not those, who have not as yet given this information, be kind enough to do so?

Associate Editors.—The supporters of this Journal will be pleased to welcome, as associate editors, those gentlemen whose names now appear on the title page of the work.

It might be expected that, on this occasion, some complimentary allusions to them should be made, but their highest eulogy is, that this is unnecessary. Their record is familiar to all, and their reputation is already established.

THE SOUTHERN PLANTER AND FARMER, RICHMOND, VA. This invaluable agricultural monthly is particularly commended to all who are interested in the true basis of enduring prosperity—the scientific *nurture*, as well as tillage of the soil.

The work is edited with great ability, and its contents are highly instructive to the general, as well as to the agricultural reader. Scientific articles are wisely associated, in its pages, with the records of valuable experience and the wealth of practical suggestion. It has been received and examined since the date of its republication, and this estimate of it is based upon experience, in regard to its value. Mr. Ch. B. Williams, the venerable and distinguished editor and proprietor, is familiar to all who are familiar with scientific agriculture, and Mr. W. L. Hill, the general agent of the Planter, is not surpassed in his department by any one, for his energy and capacity, and for great versatility in all of those resources which bring, to such a work, increasing value and deserved success. The Planter is published at the low rate of $2 per annum.

THE KENTUCKY STATE MEDICAL SOCIETY.—The editor of this Journal desires, at the first possible opportunity, to return his sincere thanks to the Kentucky State Medical Society, for the invitation extended to him to remove the Journal to this State and for the pledge of support recently and publicly given, in official action at Danville, Ky. The Journal will in future be published in this city, and the editor will endeavor to make it not unworthy of the compliment bestowed and of the support promised.

NOTICES.

Circular.—The undersigned will take pleasure in supplying his Professional Brethren with vaccine virus, as heretofore, on the receipt of two postage stamps. At the same time, he is desirous of aiding the Female Orphan Asylum of this city, a part of whose inmates are sad legacies of the recent civil strife. He, therefore, proposes to all who can afford it, to remit one dollar for each crust, to be applied to the uses of this touching charity.

The virus, he proposes to distribute, has been cultivated more than twelve years. It is very active, and perfectly protective; the crust may almost always be removed on the fourteenth day, and never leaves a deep ulcer.

Should any abnormal symptoms be produced, or should it fail altogether, he will be glad to be notified.

James Bolton, M. D., Richmond, Va.

Special Notice.—It often happens to the busy Practitioner that, while he is unable to make a careful review of the history and pathology of any disease which has been presented to him in an important or unusual form, he can easily spare the time necessary for communicating by letter all of the interesting facts, in connection with one or more of his cases.

It will have been observed, in recent issues of this Journal, that this has been done, and such letters having been published, have aroused interest and attention; indeed, there is no portion of a Journal which is more generally and profitably read. It is requested, therefore, that all Physicians, if indisposed to write a formal article, based upon their practice, will communicate by letter an account of all that may have interested them. Such letters are fully appreciated by readers, and will be published with pleasure.

Dr. E. H. M. Sell, 47 West 29th street, New York, is a General Business Agent of this Journal, and, as such, is authorized to contract for Advertisements and Subscriptions, and to collect and receipt for the same.

Notice.—Physicians who are members of Medical Associations or Societies are respectfully requested to send copies of all interesting reports or abstracts of instructive debates, etc., for publication in this Journal.

For advantageous club rates and premiums, to all desiring to support this Journal, see advertisement.

To PHYSICIANS.—Arrangements have been made whereby any American Medical book, or American edition of any Foreign Medical book, will be sent, free of expense, by mail or express, ON RECEIPT OF THE LOWEST SUM charged by the publishers. Physicians desiring any work will be supplied by the Editor of this Journal on these terms. Many of the works desired, with the respective price of each, will be found in the advertising pages ef this Journal. Where the work is not specified, information as to its price may be obtained by a letter addressed to the editor.

All American Journals and reprints of Foreign Journals will be furnished to subscribers at 15 per cent. below cost.

For the convenience of the supporters of this Journal, instruments of all kinds, and medicines, will be purchased without charge, and sent as may be desired, by mail or express.

SHORT ARTICLES.—It is requested, that articles contributed for this work, should be brief and practical. With few exceptions, long communications are distateful to readers and are generally neglected; it is difficult to make room for them. Papers of merit are retained for many months, when, if the valuable truths contained were briefly presented, they would be promply published. It is the desire of the Editor of this Journal to increase, relatively, the Eclectic Department, and, to effect this, original papers of limited extent are preferable.

WHERE subscribers reside in cities, they will oblige the Editor of this Journal by forwarding their local address. Without such information, the Journals very commonly lie neglected or undelivered, at the different Postoffices.

CORRESPONDENTS will save themselves much annoyance and the Editor much regret by preparing their manuscript carefully; writing with legibility, using but one side of the paper, and dividing the text into paragraphs.

THIS JOURNAL is published as a Monthly octavo of ninety-six pages. It is now the largest Medical Monthly issued in America. The terms of the Journal have been changed. See notice on the cover.

MR. W. P. HOLLOWAY, of Louisville, Ky., is a General Business Agent of this Journal, and, as such, is authorized to attend to all business connected with the work.

MR. JOHN DUNLOP, of Richmond, Va., is authorized to collect all sums due to this Journal, prior to January, 1868.

ONE DOLLAR will be paid for every October (1867) number of this Journal.

NOTICES TO SUBSCRIBERS, ADVERTISERS, CONTRIBUTORS, AND CORRESPONDENTS.

Receipts for all amounts transmitted, will be sent in the Journal, for the succeeding month.

All communications sent by mail should be directed to Dr. E. S Gaillard, Locked Box 29, Louisville, Ky.

European packages, sent through Trubner & Co., 60 Paternoster Row, London, or J. B. Bailliere et Fils, Rue Hauterfeuille, Paris, will be safely forwarded.

It is expected that all expenses will be paid on packages, foreign and domestic, sent to this Journal.

Contributions for publication must bear the signature of the writer, and must be communicated to this Journal exclusively.

Original articles should be written on one side only of each sheet and the manuscript should be, by the sign ¶, divided into paragraphs.

Contributors can have forwarded to them, at a small expense, one hundred or more copies of their articles, in pamphlet form, provided the order to this effect is received before the period of publication.

The Journal will be sent a second time, when not received one month after the usual period of its appearance.

Rejected manuscript will be returned, if desired, by mail or express at the expense of the writer.

Each subscriber should give at length his name, Postoffice, County and State.

THE Postoffice Department has introduced the Express arrangement in regard to all registered letters. Such letters are placed in the hands of route-agents, and each route-agent is required to take a receipt from the next agent, into whose hands registered matter is delivered. This course is adopted, consecutively, from the office at which the letter is mailed, until it reaches the office of delivery, where a final receipt is given by the person for whom the package is intended. It is thus evident, that all risk in sending registered letters has been removed. Subscribers are requested to transmit money, either by postoffice order, or by registered letters, as may be most convenient.

BOOKS AND JOURNALS RECEIVED.

Atlas of Venereal Diseases. By A. Cullerier, Surgeon to the Hopital du Midi, Paris. Translated from the French by Freeman J. Bumstead, M. D. New York: Parts II and III: Henry C. Lea, Philadelphia, 1868. From the publishers.

Electro-Physiology and Therapeutics. Being a Study of the Electrical and other Physical Phenomena of the Muscular and other Systems during Health and Disease. By Chas. E. Morgan, A. B., M. D. New York: Wm. Wood & Co., 1868. From the publishers.

Therapeutics and Materia Medica. A Systematic Treatise on the Action and Uses of Medicinal Agents, including their Description and History. By Alfred Stille, M. D., Philadelphia, 1868. From the publishers.

A Manual of the Dissection of the Human Body. By Luther Holden, F. R. C. S., St. Bartholomew's Hospital, London. R. M. Dewitt, Publisher, New York, 1868. From the editor and publisher.

The Principles and Practice of Laryngoscopy and Rhinoscopy. By Antoine Ruppaner, M. D., M. A. New York: A. Simpson & Co., 1868. From the publishers.

Pathological Anatomy of the Female Sexual Organs. By Julius M. Klob, M. D., Vienna. New York: Moorhead, Simpson & Bond, 1868. From the publishers.

Second Annual Report of the Metropolitan Board of Health of the State of New York, 1867. New York: 1868. From Dr. E. Harris, Registrar.

Clinical Observations in the Ophthalmic Department of the Rochester City Hospital. By Charles E. Rider, M. D., Rochester, N. Y. From the author.

Rapports Du Jury International. Paris.

The following domestic Exchanges are regularly received: The Atlanta Medical and Surgical Journal, Ga.; Boston Medical and Surgical Journal, Mass.; Braithwaite's Retrospect, New York; Buffalo Medical and Surgical Journal, New York; Chicago Medical Journal, Illinois; Chicago Medical Examiner, Illinois; Cincinnati Lancet and Observer, Ohio; Druggist's Circular and Chemical Gazette, New York; Journal of Applied Chemistry, Philadelphia, Pa.; London Lancet (reprint), New York; New York Medical Record, New York; New York Medical Journal, New York; New Orleans Journal of Medicine, La.; Pacific Medical and Surgical Journal, San Francisco, California; St. Louis Medical and Surgical Journal, Missouri; The American Journal of Pharmacy, Philadelphia, Pa.; The American Journal of Medical Sciences, Philadelphia, Pa.; The Dental Cosmos, Philadelphia, Pa.; The Detroit Review, Mich.; The Medical and Surgical Reporter, Philadelphia, Pa.; The St. Louis Medical Reporter, Mo.; The Western Journal of Medicine, Indianapolis, Ind.; The Journal of Materia Medica, Lebanon, N. Y.; The Leavenworth Medical Herald, Kansas; The American Journal of Dental Science, Baltimore, Md.; The Quarterly Journal of Psychological Medicine, N. Y.; The Medical Gazette, N. Y.; The Cincinnati Medical Repertory; Boston Journal of Chemistry.

FOREIGN EXCHANGES.

The London Lancet; The Ophthalmic Review, London, Eng.; The Medical Mirror, London; The Canada Medical Journal, Montreal; The Dublin Quarterly Journal of Medical Sciences, Ireland; Le Mouvement Medicale, Paris, France; Journal de Medicine, Paris, France; Revue de Therapeutique Medico Chirurgicale, Paris, France; Revue Medicale, Paris, France; The Medical Press and Circular, Dublin, Ireland; Archives de Physiologie, Paris, France; Revista Medico Quirurgica, New York.

MISCELLANEOUS.

The American Naturalist, Salem, Mass.; De Bow's Review, 80 Broadway, N. Y.; The Planter, Richmond, Va.; Littell's Living Age, Boston, Mass.; Southern Home Journal, Baltimore, Md.; Southern Boys' and Girls' Monthly, Richmond, Va.; The Atlantic Monthly; Every Saturday; New York Church Journal; Southern Review; Scientific American.

AGENTS.

The following gentlemen are authorized to act as Agents for this Journal: Messrs. Lindsay and Blakiston, 25 South Sixth street, Philadelphia, Pa.; Mr. W. P. Holloway, Louisville, Ky.; Dr. W. A. B. Norcom, Edenton, N. C.; Raoul & Linah, Charleston, S. C.; Dr. E. H. M. Sell, 47 West 29th st., New York; Dr. H. L. Barton, Lexington, Va.

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